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AN ACCOUNT OF EXPERIMENTS MADE BY A BOARD OF UNITED STATES NAVAL ENGINEERS WITH SCREW PROPELLERS OF DIFFERENT MATERIAL AND DIMENSIONS, APPLIED TO THE UNITED STATES FISH COMMISSION'S STEAMER "LOOKOUT," WITH THE HULL COPPERED AND NOT COPPERED.

By Chief-Engineer ISHERWOOD, U. S. Navy.

(Continued from page 49.)

In the following Table No. 1 will be found the data of the experiments. The quantities therein given are so fully described as to need no further explanation.

The friction of the engine, *per se*, that is to say, the pressure required to work the unloaded engine, is taken at 2 pounds per square inch of the pistons of the two cylinders. The remainders of the indicated pressures, after deduction of these two pounds, are the net pressures upon the pistons.

In order to obtain a single expression for the indicated and for the net pressures, they have been reduced to what they would have been had the engine consisted of the large cylinder alone.

Explanation of Table No. 2.

In Table No. 2 will be found the distribution of the power during each of the experiments whose data are recorded in Table No. 1, the capital letters at the heads of the columns, in both tables, indicating experiments with the screws designated by the same letters.

This distribution is necessary in order to understand what portions of the indicated horses-power developed by the engine are expended in the different operations connected with the propulsion of a vessel by a screw.

Of the indicated horses-power a portion is consumed in working the engine, *per se*, that is to say, the unloaded engine; and this portion must first be deducted, because, until the friction of the engine is overcome, no power can be applied to the screw, or externally of the engine; for it is obvious, that until the friction of the engine itself is counterbalanced, the piston cannot move.

After the deduction of this power, the remainder of the horses-power, called the net horses-power, is applied to the crankpin and does external work. A friction attends the net power, additional to that of the unloaded engine and proportional to the net power, let the latter be what it may; and the power required to overcome this friction is the power absorbed by the friction of the load, the articulations of the engine moving under the net pressure producing greater friction on them than is due to the mere weight of the moving parts and to the pressure of the packings.

Then, there are the horses-power expended in overcoming the skin or surface resistance of the screw blades experienced from the water in which they move. This power can be calculated independently when the data are known.

There still remain to be determined the portions of the net horses-power expended in the slip of the screw and in the propulsion of the vessel. These are ascertained as follows: The sum of the powers expended in overcoming the friction of the load and in overcoming the resistance of the water to the surface of the screw blades being deducted from the net horses-power, the remainder is divided between the power expended in the slip of the screw and in the propulsion of the vessel in the ratio of the speeds of the two; the pressure exercised by the screw forward in propelling the vessel, and backward upon the receding mass of water, constituting the slip of the screw, being the same. Hence, if the aforesaid remainder of power be mul-

SCREW EXPERIMENTS.

By Chief Engineer ISHERWOOD, U. S. Navy.

TABLE No. 1, Containing the Data of the Experiments made in 1880, in the Potomac River, by a Board of United States Naval Engin
States Fish Commission's Steamer "Lookout," for the Purpose of Ascertaining the Best Proportio

		Hull not coppered.		
		A	*B*	*C*
	Date of experiments (a pair of runs on each day),	January 19, 20 & 21.	March 26, 29 & 30.	January 7, 10 & 15.
	Number of runs over the base of 11⅛ geographical miles,	6·	6·	6·
SPEED.	Number of double strokes made by the engine's pistons, and of revolutions made by the screw, per minute,	152·3085	158·1988	158·8560
SPEED.	Speed of the vessel per hour, in geographical miles of 6086 feet,	9·5346	10·0974	9·6264
SPEED.	Slip of the screw, in per centum of its speed,	24·4072	13·6772	18·0439
CYLINDER PRESSURES.	Indicated pressure on the piston of the small cylinder, in pounds per square inch,	37·9960	39·9330	35·906
CYLINDER PRESSURES.	Net pressure on the piston of the small cylinder, in pounds per square inch,	35·9960	37·9330	33·906
CYLINDER PRESSURES.	Indicated pressure on the piston of the large cylinder, in pounds per square inch,	8·3004	8·7000	7·618
CYLINDER PRESSURES.	Net pressure on the piston of the large cylinder, in net pounds per square inch,	6·3004	6·7000	5·618
CYLINDER PRESSURES.	Indicated pressure that would have been upon the piston of the large cylinder had the indicated pressure upon the piston of the small cylinder been reduced in the ratio of the net area of the piston of the large cylinder to the net area of the piston of the small cylinder, and the quantity thus obtained added to the indicated pressure upon the piston of the large cylinder,	21·9103	23·0038	20·4793
CYLINDER PRESSURES.	Net pressure that would have been upon the piston of the large cylinder had the net pressure upon the piston of the small cylinder been reduced in the ratio of the net area of the piston of the large cylinder to the net area of the piston of the small cylinder, and the quantity thus obtained added to the net pressure upon the piston of the large cylinder,	19·1939	20·2874	17·7629
HORSES-POWER.	Indicated horses-power developed in the small cylinder,	52·4763	57·6466	51·7217
HORSES-POWER.	Indicated horses-power developed in the large cylinder,	32·0041	35·0624	30·6356
HORSES-POWER.	Aggregate indicated horses-power developed by the engine,	84·4804	92·7090	82·3573
HORSES-POWER.	Net horses-power developed in the small cylinder,	49·7141	54·7594	48·8407
HORSES-POWER.	Net horses-power developed in the large cylinder,	24·2926	27·0021	22·5927
HORSES-POWER.	Aggregate net horses-power developed by the engine,	74·0067	81·7615	71·4334

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880, in the Potomac River, by a Board of United States Naval Engineers wit
MER "Lookout," for the Purpose of Ascertaining the Best Proportions of Scr

	Hull not coppered.			
	A	*B*	*C*	
. . . .	January 19, 20 & 21.	March 26, 29 & 30.	January 7, 10 & 15.	April
. . .	6·	6·	6·	6
of revolutions made by	152·3085	158·1988	158·8560	153
. . . .	9·5346	10·0974	9·6264	10
. . .	24·4072	13·6772	18·0439	20
ds per square inch, .	37·9960	39·9330	35·906	45
square inch, .	35·9960	37·9330	33·906	43
ds per square inch, .	8·3004	8·7000	7·618	9
per square inch, .	6·3004	6·7000	5·618	7
large cylinder had the en reduced in the ratio net area of the piston of o the indicated pressure . . .	21·9103	23·0038	20·4793	25
ge cylinder had the net d in the ratio of the net f the piston of the small pressure upon the piston . . .	19·1939	20·2874	17·7629	23
. . . .	52·4763	57·6466	51·7217	63
. . . .	32·0041	35·0624	30·6356	36
. . . .	84·4804	92·7090	82·3573	100
. . . .	49·7141	54·7594	48·8407	60
. . . .	24·2926	27·0021	22·5927	29
. . . .	74·0067	81·7615	71·4334	90

EERS WITH THE DIFFERENT SCREW PROPELLERS APPLIED TO THE UNITED
NS OF SCREW FOR THAT VESSEL.

		Hull coppered.	
D	***E***	***F***	***G***
April 1, 2 & 3.	June 23, 26 & 28.	October 19 & 20.	September 24 & 25.
6·	6·	4·	4·
153·4601	140·0500	143·5402	143·4125
10·4190	10·1928	9·9390	9·7493
20·1542	17·9739	21·9620	12·7150
45·7104	44·7881	39·6325	36·5380
43·7104	42·7881	37·6325	34·5380
9·5172	10·7662	9·2050	8·2954
7·5172	8·7662	7·2050	6·2954
25·8904	26·8090	23·4011	21·3831
23·1740	24·0926	20·6847	18·6667
63·6080	56.8784	51·5854	47·5153
36·9732	38·1705	33·4487	30·1167
100·5812	95·0489	85·0341	77·6320
60·8249	54·3385	48·9822	44·9144
29·2034	31·0797	26·1812	22·8556
90·0283	85·4182	75·1634	67·7700

tiplied by the speed of the slip expressed in fractions of the axial speed of the screw, the product will be the power expended int he slip, which being subtracted from the above remainder leaves the residue as the power expended in the propulsion of the vessel.

On these general principles, the corresponding quantities in Table No. 2 have been calculated. For facility of reference, the quantities have been grouped and the lines containing them numbered.

Line 1 contains the dates of the experiments. Line 2 gives the speeds of the vessel per hour, in geographical miles of 6086 feet. Line 3 gives the slips of the screws in per centum of their speed, calculated from their pitches into the number of their revolutions made per hour, and from the speeds of the vessel on line 2. On line 4 are the thrusts of the screws in pounds during each experiment; these quantities are what would have been given by a dynamometer had one been applied directly to the screw shaft and fulcrumed on the vessel. They are calculated from the quantities on line 19, by multiplying the latter by 33,000 and dividing the product by the speed of the vessel in feet per minute. Line 5 contains the number of double strokes made per minute by the pistons of the engine, and of revolutions made per minute by the screw. The quantities on lines 1, 2, 3 and 5 are taken from Table No. 1 and added for the sake of completeness to Table No. 2.

Distribution of the Indicated Horses-Power.

Lines 13 to 19, both inclusive, show the distribution of the indicated horses-power among the operations contingent on the propulsion of the vessel by the experimental screws. Line 13 contains the indicated horses-power, the quantities being taken from Table No. 1; and line 15 contains the net horses-power applied to the crankpin, also taken from Table No. 1. Line 14 contains the horses-power expended in working the engine, *per se*, or unloaded, the quantities being the difference between those on lines 13 and 15; they can, however, be obtained independently by calculation, the data being the speed of the piston per minute in feet obtained from line 5, the area in square inches of the piston of the large cylinder alone, and the pressure on line 7. Line 16 contains the horses-power absorbed by the friction of the load; these quantities are obtained by multiplying those on line 15 by 0·075, that fraction being the coefficient of the friction of the load. Line 17 gives the horses-power expended in overcoming the

resistance of the water to the surface of the screw blades. These quantities have been calculated on the assumption that a square foot of the helicoidal surface of the screws, moving in its helical path with a speed of 10 feet per second, has a resistance of 0·45 pound, which resistance increases or decreases in the ratio of the square of the helical speeds. Line 18 contains the horses-power expended in the slips of the screws, these quantities are calculated by multiplying the remainders of the quantities on line 15 after subtraction of the sum of the quantities on lines 16 and 17, by the quantities on line 3 expressed in fractions of the axial speeds of the screws. Line 19 gives the horses-power expended in the propulsion of the vessel, which power is all that is utilized of the entire power developed by the engine. The quantities are the remainders of those on line 15 after subtraction of the sum of the quantities on lines 16, 17 and 18.

Distribution of the Indicated Pressure on the Piston.

Lines 6 to 12, both inclusive, show the distribution of the indicated pressure. A single expression for this pressure being necessary, and the engine having two compounded cylinders—a small one and a large one—the desired single expression was obtained by reducing the experimental indicated pressure per square inch on the piston of the small cylinder, in the ratio of the areas of the pistons of both cylinders, and adding the quantity thus obtained to the experimental indicated pressure per square inch on the piston of the large cylinder. These are the quantities on line 6, and are taken from Table No. 1. Line 7 contains the pressure per square inch of the large piston alone, required to work the engine, *per se*. With both cylinders in use, this pressure was taken at 2 pounds per square inch of the pistons, and the equivalent, if applied to the piston of the large cylinder alone, was obtained by dividing 2 by the ratio of the areas of the two pistons and adding the quotient to 2, which produced the constant 2·7164 pounds per square inch, for the piston of the large cylinder alone, on line 7. Line 8 gives the net pressures applied to the crank-pin, in pounds per square inch of the piston of the large cylinder alone; these quantities are the remainders of those on line 6 after subtraction of those on line 7. Line 9 contains the pressures, per square inch of the piston of the large cylinder alone, absorbed by the friction of the loads; these quantities are the products of those on line 8 by the fraction 0·075 which is the coefficient for the friction.

Line 10 contains the pressures per square inch of the piston of the large cylinder alone expended in overcoming the resistance of the water to the surface of the screw blades. These quantities are the same proportion of those on line 8, that the quantities on line 17 are of those on line 15. Line 11 contains the pressures per square inch of the piston of the large cylinder alone expended in the slip of the screw. These quantities are the same proportion of those on line 8, that the quantities on line 18 are of those on line 15. Line 12 contains the pressures per square inch of the piston of the large cylinder alone, expended in the propulsion of the vessel. These quantities are the same proportion of those on line 8, that the quantities on line 19 are of those on line 15. Or, the quantities on line 12 are what remain after subtraction from those on line 8 of the sum of the quantities on lines 9, 10, 11 and 12.

The quantities on lines 20, 21, 22 and 23 are the per centum, respectively, which the quantities on lines 16, 17, 18 and 19 are of those on line 15. The quantities on lines 20, 21, 22 and 23 are also the per centum, respectively, which the quantities on lines 9, 10, 11 and 12 are of those on line 8. The quantities on lines 20, 21, 22 and 23 show, centesimally, the distribution of the net pressure and of the net power applied to the crankpin.

The quantities on line 23 express the relative utilizations of the experimental screws; that is to say, they express the per centum of the net power applied to the crankpin which the screws apply to the propulsion of the vessel. These utilizations are correct if the distribution of the net power be correct, but not otherwise. It is, therefore, of importance that the relative utilizations of the screws be obtained directly from data devoid of doubt. The quantities on line 24 express such results, and are obtained by dividing the cubes of the speeds of the vessel on line 2 by the net horses-power applied to the crankpin—line 15. The cube of the speed of the vessel expresses the useful work done, and the net horses-power expresses the power expended in doing it; the latter divided into the former gives the work done *pro rata* to power expended, therefore the larger the quotients of the divisions, that is, the larger the quantities on line 24, the greater will be the utilizations of the screws.

The quantities on line 24 afford the means of demonstrating whether the distribution of the power is correct; if it be, then the quantities on line 23 will have the same ratio to each other that

the quantities on line 24 have to each other. To find if this equality exists, the quantities on line 24 were divided into those on line 23, and the quotients are given on line 25. If these quotients were equal, then the distribution of the power would be exact; if unequal, it is inexact to the extent of the inequality. When these quotients are examined, there must be recollected that they consist of two sets—one for the uncoppered hull (columns *A*, *B*, *C*, *D* and *E*), the other for the coppered hull (columns *F* and *G*); consequently, the equality referred to must be looked for only between the experiments of each set.

The fact that in the experiments with the coppered hull, its resistance was less than when it was uncoppered, makes its speed higher with a given net power than when uncoppered; and the quotients of the division of the cubes of the speeds of the vessel by the net horses-power will consequently be higher with the coppered than with the uncoppered hull. These higher quotients (experiments *F* and *G*, line 24) divided into the corresponding utilizations, or quantities on line 23, give accordingly less results on line 25 for experiments *F* and *G* than for the remaining experiments.

The near equality of the quantities on line 25 for experiments *A*, *B*, *C*, *D* and *E* with the uncoppered hull, and for experiments *F* and *G* with the hull coppered, prove the principle and the constants employed in calculating the distribution of the power to be correct. No nearer approach to equality than these quantities present could be obtained from delicate laboratory experiments. With this confirmation, the data and results of these screw trials may be accepted as absolutely exact.

Results.

The experiments allow the difference between the resistance of the of the hull when coppered and when uncoppered to be determined. The mean speed of the five experiments *A*, *B*, *C*, *D* and *E*, with the uncoppered hull (line 2, Table No. 2) was 9·9741 geographical miles per hour; and the mean thrust of the screw, expressing the resistance of the vessel, was 1814·1283 pounds (line 4, Table No. 2). The mean speed of the two experiments *F* and *G*, with the hull coppered, was 9·8442 geographical miles per hour, and the mean thrust of the screw was 1612·0248 pounds. The latter quantity increased in the ratio of the squares of the speeds 9·9741 and 9·8442 geographical miles per hour, becomes 1654·8370 pounds, which compared with the 1814·1283

pounds, shows that leaving off the copper from the exterior immersed surface of the hull, increased its resistance 12·5372 per centum over its resistance when coppered.

The experiments also allow the determination to be made of the power of the speed in which the resistance of the hull increased for the interval between the extreme experimental speeds of 9·5346 (experiment *A*) and 10·4190 (experiment *D*) geographical miles per hour. For this purpose, the mean of the speeds and of the corresponding resistances of the hull in experiments *A*, *C* and *G*, in which the speed did not vary enough to sensibly influence the result, will be taken for one speed; the mean of the speeds and of the corresponding resistances of the hull in experiments *B*, *E* and *F*, in which the speed did not vary enough to sensibly influence the result, will be taken for another speed; and the speed and resistance of the hull in experiment *D* will be taken for the last speed. The resistances of the hull, line 4, in experiments *F* and *G*, during which the hull was coppered, will be equated for those in the remaining five experiments during which the hull was uncoppered, by multiplying them by the 1·125372 in the immediately preceding paragraph.

The mean speed of experiments *A*, *C* and *G* was

$$\left(\frac{9\cdot5346+9\cdot6264+9\cdot7493}{3}=\right)9\cdot6368$$ geographical miles per hour;

and the mean corresponding resistance of the hull was

$$\left(\frac{1650\cdot9925+1688\cdot6734+1580\cdot2261\times1\cdot125372}{3}=\right)$$ 1706·0027 lbs.

The mean speed of experiments *B*, *E* and *F* was

$$\left(\frac{10\cdot0974+10\cdot1929+9\cdot9390}{3}=\right)10\cdot0764$$ geographical miles per hour,

and the mean corresponding resistance of the hull was

$$\left(\frac{1851\cdot0375+1909\cdot3913+1643\cdot8234\times1\cdot125372}{3}=\right)$$ 1870·1139 lbs.

The speed of experiment *D* was 10·4190 geographical miles per hour, and the corresponding resistance of the hull was 1970·5470 pounds.

The experiments with the closely approximating speeds have been grouped and their mean results taken, because the greater the number of observations included the nearer accurate is the final determination.

From the above appears it that the resistance of the hull for the

increase of speed from 9·6368 to 10 0764 geographical miles per hour was in the ratio of the 2·0590 power of the speed. Had the resistance for the speed of 10·0764 miles been 1865·1949 pounds instead of 1870·1139 pounds, it would have increased exactly as the square of the speed; the difference of 4·9190 pounds is only 0·0026 per centum of the experimental resistance in excess.

There also appears from the above that the resistance of the hull for the increase of speed from 10·0764 to 10·4190 geographical miles per hour was in the ratio of the 1·5646 power of the speed. Had the resistance for the speed of 10·4190 miles been 1999·4435 pounds instead of 1970·5470 pounds, it would have increased exactly as the square of the speed; the difference of 28·8965 pounds is only 1·4664 per centum deficit of the experimental resistance.

Finally, there appears likewise from the above, that the resistance of the hull for the increase of speed from 9·6368 to 10·4190 geographical miles per hour was in the ratio of the 1·8472 power of the speed. Had the resistance of the speed of 10·4190 miles been 1994·1875 pounds instead of 1970·5470 pounds it would have increased exactly as the square of the speed; the difference of 23·6405 pounds is only 1·1997 per centum in deficit of the experimental resistance.

It is obvious that the slight differences found between the experimental resistances and what these should be according to the ratio of the squares of the speeds are so small that they may be fairly attributed to errors of observation and assumption. In fact, the data evidently cannot be relied on as to any greater accuracy, and the conclusion is, therefore, warranted that within the experimental speeds the resistance of the hull of the *Lookout* at different speeds varied as the squares of the speeds.

In experimenting with different propellers for the purpose of ascertaining their relative economic merits, that is to say, the relative per centage which they apply to the vessel propulsively of the power they receive from the engine, care should be taken that in every experiment the same speed of vessel be maintained, which equality eliminates all question as to the law of variation of resistance of the hull in function of its speed. If that resistance increase as the squares of the speeds then the accuracy of the results is not affected by variations of the vessel's speed because the slip of the same propeller would remain constant at all speeds, the reaction it obtains from the water varying always as the square of its revolutions in equal time. But if the resist-

ance of the vessel increase in a higher ratio than the square of the speed, then the slip of the same propeller would be proportionally greater at higher than at lower speeds of the vessel, and the propeller experimented with at higher speed would, relatively to another experimented with at lower speed, give a less economic result than if it were experimented with at the same speed. At the higher speed there would be proportionally more power expended in the slip, and consequently more power expended in overcoming the resistance of the water to the surfaces of the propeller moving in it. In other words, at the higher speed of vessel the same propeller will apply to it propulsively a less percentage of the power received from the engine than at the lower speed, if the resistance of the vessel increase in a higher ratio than the square of its speed.

Assuming, in the case of the experiments with the screws of the "Lookout," that the resistances of the vessel at the different speeds varied as the square of those speeds, an assumption warranted by the experimental results, the following would be the resistances of the coppered vessel for a speed of 10 geographical miles per hour, deduced from the experimental resistances at the experimental speeds in the ratio of the squares of these speeds to the square of 10:

Screw.	Resistance of vessel in pounds at 10 miles per hour.
A	1613·78
B	1613·24
C	1619·28
D	1613·02
E	1633·06
F	1664·06
G	1662·54
Mean,	1631·28

In the above determinations the experimental resistances with screws *A*, *B*, *C*, *D* and *E* have been reduced in the ratio of 1·125372 to 1, as the vessel in those cases was not coppered, that ratio expressing the effect of the coppering.

An examination of line 23, Table No. 2, shows that screws *B*, *C*, *D* and *E* had the highest economic efficiency; also, that their economic results were sensibly the same, as they all applied propulsively to the *uncoppered* vessel, 70 per centum of the net horses-power developed by the engine, that is to say, 70 per centum of the power applied to the

crank-pin. And, if the proper correction be made for the diminution of the vessel's resistance due to coppering, these screws would have usefully applied to the *coppered* vessel 71·3117 per centum of the net horses-power developed by the engine. Now, screws *B*, *C*, *D* and *E*, though having the same diameter and number of blades, varied largely in pitch and in the fraction used of the pitch, the pitches being 7·500, 7·500, 8·625 and 9 feet, and the respective fractions used of these pitches being 0·5283, 0·2703, 0·1994, 0·3549, showing that for a given diameter the pitch and fraction used of the pitch may vary considerably without affecting the economic performance of the screw. This arises from the fact that the losses of useful effect by the screw are, 1st, that which is due to its slip, and 2d, that which is due to the resistance of the water on the surface of its blades; including in 2d the direct resistance of the forward edges of the blades. And that the 1st loss cannot be diminished without the 2d loss being increased, *pari passu* and *vice versa*. The slip depends on the proportion of the acting surface of the screw in a given time, relatively to the resistance of the vessel, and can only be lessened by adding more of such surface, which, with a given diameter of screw, can be effected by either diminishing the pitch or increasing the fraction used of the pitch. But, as the loss by the resistance of the water on the screw surface is in direct proportion to that surface multiplied by the square of its velocity, any increase of the acting surface is necessarily accompanied by increased loss due to this cause. The two kinds of loss are found experimentally to be so balanced in nature that within very considerable limits whatever can be diminished of one kind is attended by an almost exactly equivalent increase of the other kind.

Screw *G* applied to the coppered vessel, and having the same diameter as screws *B*, *C*, *D* and *E*, with the pitch of 7·9 feet, which lies within the limits of their pitches, gave a utilization of 69·8750 per centum of the net power developed by the engine, instead of the utilization 71·3117 per centum, that would have been given by those screws had they been applied to the coppered vessel. This shows an inferiority of $\left(\frac{71{\cdot}3117-69{\cdot}8750\times100}{71{\cdot}3117}=\right)$ 2·0147 per centum for screw *G*, which is probably owing to the excessive fraction used of its pitch, namely, 0·5736, the largest fraction used in the cases of screws *B*, *C*, *D* and *E* being 0·5283. The increased power required to overcome the resistance of the water to the surfaces of screw *G*, owing to the increased

SCREW EXPERIMENTS.

By Chief Engineer ISHERWOOD, U. S. Navy,

TABLE No. 2, Containing the Distribution of the Piston Pressures and of the Indicated Horses-Power during the Experiments made in Engineers with the Different Screw Propellers Applied to the Fish Commission's Steamer "Lookout," for the Purpose of Ascer

The piston pressures in this table are the pressures that would have been upon the piston of the large cylinder had the pressures upon the piston of the small cylinder the net area of the piston of the small cylinder, and the quantities thus obtained added to the observed pressures on the piston of the large cylinder. The capital letter given in the columns of Table No. 1, with the same letter headings.

No. of Line.				Hull not coppered.		
				A	*B*	*C*
1			Date of experiments,	January 19, 20 & 21.	March 26, 29 & 30.	January 7, 10 & 15.
2			Speed of the vessel per hour, in geographical miles of 6086 feet,	9·5346	10·0974	9·6264
3			Slip of the screw, in per centum of its speed,	24·4072	13·6772	18·0439
4			Thrust of the screw, in pounds,	1650·9925	1851·0875	1688·6734
5			Double strokes of engine's pistons, and revolutions of the screw made per minute,	152·3085	158·1988	158·8560
6	Distribution of the indicated horses-power.	Distribution of the indicated pressure on the piston.	Indicated pressure on the piston, in pounds per square inch,	21·9103	23·0038	20·4793
7			Pressure required to work the engine, *per se*, in pounds per square inch of piston,	2·7164	2·7164	2·7164
8			Net pressure applied to the crankpin, in pounds per square inch of piston,	19·1939	20·2874	17·7629
9			Pressure absorbed by the friction of the load, in pounds per square inch of piston,	1·4395	1·5216	1·3322
10			Pressure expended in overcoming the resistance of the water to the surface of the screw blades, in pounds per square inch of piston.	1·1537	2·2522	1·2704
11			Pressure expended in the slip of the screw, in pounds per square inch of piston,	4·0518	2·2586	2·7355
12			Pressure expended in the propulsion of the vessel, in pounds per square inch of piston,	12·5489	14·2550	12·4248
13		Absolute.	Indicated horses-power developed by the engine,	84·4804	92·7090	82·3573
14			Horses-power expended in working the engine, *per se*,	10·4737	10·9475	10·9239
15			Net horses-power applied to the crankpin,	74·0067	81·7615	71·4334
16			Horses-power absorbed by the friction of the load,	5·5505	6·1321	5·3575
17			Horses-power expended in overcoming the resistance of the water to the surface of the screw blades,	4·4483	9·0767	5·1089
18			Horses-power expended in the slip of the screw,	15·6225	9·1025	11·0008
19			Horses-power expended in the propulsion of the vessel,	48·3854	57·4502	49·9662
20		Proportional.	Per centum of the net horses-power applied to the crankpin, absorbed by the friction of the load,	7·5000	7·5000	7·5000
21			Per centum of the net horses-power applied to the crankpin, expended in overcoming the resistance of the water to the surface of the screw blades,	6·0107	11·1014	7·1520
22			Per centum of the net horses-power applied to the crankpin, expended in the slip of the screw,	21·1096	11·1330	15·4001
23			Per centum of the net horses-power applied to the crankpin, expended in the propulsion of the vessel,	65·3797	70·2656	69·9479
24			Relative economic efficiencies of the screws, expressed by the quotient of the division of the cubes of the speeds (line 2) by the net horses-power applied to the crankpin (line 15),	11·7122	12·5049	12·4880
25			Quotients of the division of the quantities on line 23 by those on line 24,	5·5822	5·6190	5·6012

SCREW EXPERIMENTS.

By Chief Engineer ISHERWOOD, U. S. Navy,

RES AND OF THE INDICATED HORSES-POWER DURING THE EXPERIMENTS MADE IN 1
ED TO THE FISH COMMISSION'S STEAMER "LOOKOUT," FOR THE PURPOSE OF ASCERT

on the piston of the large cylinder had the pressures upon the piston of the small cylinder b
ined added to the observed pressures on the piston of the large cylinder. The capital letters

	Hull not coppered.		
	A	*B*	*C*
	January 19, 20 & 21.	March 26, 29 & 30.	January 7, 10 & 15.
6086 feet,	9·5346	10·0974	9·6264
	24·4072	13·6772	18·0439
	1650·9925	1851·0375	1688·6734
the screw made per minute,	152·3085	158·1988	158·8560
e inch,	21·9103	23·0038	20·4793
ds per square inch of piston,	2·7164	2·7164	2·7164
quare inch of piston,	19·1939	20·2874	17·7629
ds per square inch of piston,	1·4395	1·5216	1·3322
e water to the surface of the ;	1·1537	2·2522	1·2704
per square inch of piston,	4·0518	2·2586	2·7355
n pounds per square inch of	12·5489	14·2550	12·4248
	84·4804	92·7090	82·3573
,	10·4737	10·9475	10·9239
	74·0067	81·7615	71·4334
	5·5505	6·1321	5·3575
of the water to the surface	4·4483	9·0767	5·1089
	15·6225	9·1025	11·0008
el,	48·3854	57·4502	49·9662
crankpin, absorbed by the	7·5000	7·5000	7·5000
rankpin, expended in over- of the screw blades,	6·0107	11·1014	7·1520
crankpin, expended in the	21·1096	11·1330	15·4001
crankpin, expended in the	65·3797	70·2656	69·9479
d by the quotient of the di- net horses-power applied to	11·7122	12·5049	12·4880
by those on line 24,	5·5822	5·6190	5·6012

ADE IN 1880, IN THE POTOMAC RIVER, BY A BOARD OF UNITED STATES NAVAL
F ASCERTAINING THE BEST PROPORTIONS OF SCREW FOR THAT VESSEL.

cylinder been reduced in the ratio of the net area of the piston of the large cylinder to
ital letters at the head of the columns in this table designate the same experiments as are

	opered.		Hull coppered.	
	D	*E*	*F*	*G*
0 & 15.	April 1, 2 & 3.	June 23, 26 & 28.	October 19 & 20.	September 24 & 25.
4	10·4190	10·1929	9·9390	9·7493
9	20·1542	17·9739	21·9620	12·7150
4	1970·5470	1909·3913	1643·8234	1580·2261
0	153·4601	140·0500	143·5402	143·4125
3	25·8904	26·8090	23·4011	21·3831
4	2 7164	2·7164	2·7164	2·7164
9	23·1740	24·0926	20·6847	18·6667
2	1·7380	1·8069	1·5514	1·4000
4	1·0914	1·7154	1·4241	2·3233
5	4·1003	3·6973	3·8893	1·9000
8	16·2443	16·8730	13·8199	13·0434
3	100·5812	95·0489	85·0341	77·6320
9	10·5529	9·6307	9·8707	9·8620
4	90·0283	85·4182	75·1634	67·7700
5	6·7521	6·4064	5·6373	5·0827
9	4·2398	6·0816	5·1748	8·4348
8	15·9291	13·1084	14·1328	6·8982
2	63·1073	59·8218	50·2185	47·3543
0	7·5000	7·5000	7·5000	7·5000
0	4·7094	7·1198	6·8847	12·4462
1	17·6934	15·3462	18·8028	10·1788
9	70·0972	70·0340	66·8125	69·8750
0	12·5632	12·3978	13·0624	13·6733
2	5·5796	5·6408	5·1149	5·2193

fraction used of its pitch over the largest fraction used with screws *B*, *C*, *D* and *E*, was probably greater than the lessened power expended in the lessened slip consequent on the use of that greater fraction of the pitch.

Screw *A* applied to the uncoppered vessel, and screw *F* applied to the coppered vessel, having very nearly the same diameter and fraction used of the pitch, but pitches of respectively 8·4 and 9·0 feet gave the same utilizations when that of screw *A* is corrected for the greater resistance of the uncoppered vessel, showing the difference of the pitches not to have affected the economic results. The utilizations of screws *A* and *F*, when equated for the resistance of the coppered vessel, are 66·8 per centum of the net power developed by the engine, consequently these two screws of $4\frac{2}{3}$ feet diameter are

$$\left(\frac{71{\cdot}3117-66{\cdot}8000\times 100}{71{\cdot}3117}=\right)\ 6{\cdot}3267$$

per centum inferior to the four screws *B*, *C*, *D* and *E* of 5 feet diameter, the pitches and fractions used of the pitch of the former lying within the limits of those of the latter. This inferiority is caused wholly by the smaller diameter, whereby the loss of useful effect, due to the greater slip resulting from the less acting surface of screws *A* and *F*, more than counterbalances the gain due to the less resistance of the water to the less acting surface of those screws. The utilizations of screws *A* and *F* could doubtless have been much increased had the fraction used of their pitch been increased from about one-third, which it was, to one-half; or, had their pitch been decreased, retaining the same fraction of pitch.

The quantities on line 24 of Table No. 2 shows relatively the utilizations of the different screws, when the comparison is made directly by dividing the cubes of the speeds (line 2) by the net horses-power developed by the engine (line 15). The quantities on line 24 are the quotients of such divisions, and have very nearly the same proportions to each other as the quantities on line 23—considering the experiments with the uncoppered vessel separately from those with the vessel coppered—as appears from the quantities on line 25, which are the quotients of the division of those on line 23 by those on line 24. The equality of the latter quotients proves, 1st, that the distribution of the power in the different experiments as given in Table 2, lines 13 to 23, both inclusive, is correct; and 2d, that no difference in the utilizations results from the difference in the metals, cast-iron and cast-brass, of which the screws were made. The only difference that

could have resulted would have been due to the different surface textures of these two metals, whereby the resistance of the water per unit of their surface might have been different.

These experiments show what many others have shown when properly interpreted—that the utilizations of screws of different proportions of diameter, pitch and fractions used of the pitch, and of different outlines and inclinations of the blades applied to the same vessel, vary very little unless extreme variations of dimensions are resorted to. The outline and inclination of the blades appear to be absolutely without effect upon the utilization, which is controlled entirely by the diameter, pitch and fraction used of the pitch.

The experiments show also that the method of distributing the indicated horses-power developed by the engine, originally devised and long practiced by the writer, is correct, and that by its means the important question of the resistance in pounds of the hull, *per se*, of a vessel at a given speed may be ascertained indirectly quite as accurately as by the direct application of a dynamometer.

The true performance and distribution of the power of the *Lookout* with her bottom coppered, when propelled by screw *E*, deduced from all the experiments, is as follows for the speed of 10 geographical miles per hour, the slip of the screw being 16 per centum of its axial velocity (product of revolutions and pitch); the number of its revolutions, 134·1711 per minute; the indicated pressure on the piston of the large cylinder, 23·4175 pounds per square inch, that cylinder being supposed to be used alone; the thrust of the screw or resistance of the hull, 1631·28 pounds, as herein determined; the indicated horses-power developed by the engine, 79·5393, and the pressure required to work the engine, *per se*, 2·7164 pounds per square inch of the piston of the large cylinder alone.

DISTRIBUTION OF THE POWER.

	Horses-power.		Per cent. of net horses-power.
Indicated power developed by the engine,	79·5393		
Power expended in working the engine, *per se*,	9·2265		
Net power applied to the crank-pin,	70·3128	or	100·0000
Power absorbed by the friction of the load,	5·2734	"	7·5000
Power expended in overcoming the resistance of the water to the surface of the screw blades,	5·3474	"	7·6051
Power expended in the slip of the screw,	9·5507	"	13·5832
Power expended in the propulsion of the vessel,	50·1413	"	71·3117
Totals,	70·3128	or	100·0000

DISCUSSION

Of the Papers of C. P. Sandberg on "Rail Specifications and Rail Inspection in Europe," of C. B. Dudley on the "Wearing Capacity of Steel Rails in Relation to their Chemical Composition and Physical Properties," and of A. L. Holley on "Rail Patterns," at the Philadelphia Meeting of the American Institute of Mining Engineers, held at the Franklin Institute, February 17th, 1881.

(Continued from page 36.)

WILLIAM METCALF, Pittsburg, Pa.: In rising to discuss Dr. Dudley's paper, I feel somewhat as I did at the Baltimore meeting—that a "crucible" man has no right to interfere in a "Bessemer" discussion; yet having read the paper very carefully, I feel impelled to say something, for two reasons: First, because I believe Dr. Dudley is entirely on the right track, and having undertaken and partly accomplished a great work, he is entitled to the help of all who have experience in these matters; and second, because the data given force me to concur in Captain Jones' opinion that the analyses are incomplete, since they ignore sulphur, copper, nitrogen, and possibly other injurious elements.

In an experience of fourteen years, and with probably more than a hundred tests, we have never found the chemistry and the physics of crucible steel to disagree. If in any case a disagreement has appeared, it has been our invariable custom to go all over our physical tests with great care, and if we found no error, then to refer the matter back to the chemist, who has invariably found some unexpected element to account for the trouble.

It is only just to the chemist to say here, that ordinarily he is only expected to determine phosphorus, silicon and sulphur. Generally the metals, with the exception of manganese, are not looked for, although a watch is usually kept for copper and arsenic. Further, in most cases we have found our own work more liable to be faulty than the chemist's.

Having arrived then at such a degree of experience that we can predict the analysis from our tests, or our tests from the analysis, with almost absolute certainty, I can see no reason why the same results may not be attained in the Bessemer practice. But two things are

essential, neither of which we have here; first, complete analyses; and second, a record of the nature of the blow, the heat at which the ingots were bloomed, and the rails finished,—in short, a complete history of the manufacture.

This latter is quite as essential as accurate and complete analyses. Dr. Dudley ignores sulphur and copper on fair enough grounds commercially speaking, but when he announces so grave a conclusion as he has reached, in a scientific way, the omission of any elements that may affect the conclusion is hardly justifiable. His differences of phosphorus units, which I must term units of rottenness as far as phosphorus is concerned, and which I am sorry he did not name "*Units of Alloys*,"—are very small, and if sulphur and copper had been included they might have upset his conclusions altogether. The omission of nitrogen is not to be criticised in the same way, because it has not been usual to regard nitrogen in testing steels, yet I am forced to believe that nitrogen plays a very important part in Bessemer steel.

We had occasion recently to test some of the finest Bessemer steel that is made, in order to ascertain how far the Bessemer people were encroaching on the domain of the crucible steel manufacturers. In the accompanying table are the analyses of the Bessemer steel referred to, two samples of the very best foreign crucible steel, and one of the best American crucible steel.

Kinds of steel.	Carbon.	Phosphorus	Silicon.	Sulphur.	Manganese.	Copper.
Bessemer.....	0·400	0·027	0·003	trace.	.....	.....
Foreign crucible.....	1·295	0·020	0·009	trace.	0·050	.....
	0·862	0·005	0·007	none.	.....	0·0013
American crucible.....	0·960	0·021	0·033	trace.	0·077	.....

All three of the crucible steels were of exceptionally good quality. It will be observed that, according to the analysis, the Bessemer steel should have been equally good. Upon a careful test of an 0·80 carbon billet it proved to be thoroughly worthless.

The case was then referred to Professor Langley, and he attributed the trouble to oxygen, and predicted that if we would melt a 0·40 carbon billet with a little ferromanganese to remove the oxygen and bring up the carbon to 0·80, and also give a little more silicon which the steel would take from the crucible, that we would have a steel equal to the others given above. To be sure of our work, we melted

the billet, the analysis of which is given above, and produced an ingot of about 0·80 carbon, as near as the eye could determine, and that is within ·03 in such high steels. The remelted Bessemer steel was just as bad as the other, and of this we assured ourselves by the most careful and repeated tests.

Now we know oxygen was not the cause of the fault, for if it had been, the ferromanganese would have removed it. In the Bessemer manufacture immense volumes of nitrogen are blown through the molten mass, and by the evidence of all the most eminent chemists who have examined the subject, we know that nitrogen does unite with iron, that the compound is brilliantly lustrous, hard, brittle, and even friable, and that it will harden like steel. We also know that there is a peculiar lustre in all Bessemer steel, which makes it easily distinguishable by an expert from crucible or open-hearth steel, provided none of them have been overheated. Is it not more than probable, then, that nitrogen is entitled to far more serious attention than it has yet received?

Dr. Dudley classes carbon with phosphorus, silicon, and manganese in making up his units, and he may be correct in his make-up in his relative values of each, but I cannot see how he has proved his formula, nor how it is to be disproved with our present knowledge.

While maintaining that carbon in steel is the great friend of the manufacturer and the only fit alloy of iron, I must admit that when it is present in quantity with phosphorus, silicon, manganese, or sulphur in quantity, it vitalizes and makes more active all of the bad qualities of these elements, and therefore if Dr. Dudley must have the quantities of these elements in his rails which he permits in his formula, he is quite right in saying that the softest rails ought to wear the best. In regard to the wearing of wire dies we have found

Carbon,	Tungsten,
1·37.	0·78.

too soft, *i. e.*, it wore too easily. Also steel of the composition

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
1·7	0·20	0·091	0·387	0·02

was too soft. The best foreign dies contained

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
2·89	0·14	0·131	0·26	0·02

and equally good and entirely satisfactory home-made steel contained

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
2·37	0·20	0·091	0·18	0·03

In this case good wear depends upon high carbon.

In steel rolls, in which we have had some experience, we have found that very soft rolls of about 0·30 carbon wore too fast from excessive flow, taking the shape shown in the accompanying cut, the overflow sometimes amounting to more than half an inch. This cuts away the brasses and involves much redressing. Rolls of about 0·70 carbon made from the same iron, neither flow nor crack to any serious extent, and will do two to three times as much work as the softer rolls.

We know that steel flows under pressure, that the milder the steel the easier it will flow, and the easier it will shear; yet in the paper under discussion flow is disregarded entirely, although there is plain evidence of it in many of the sections given. Low shearing stress is advised as conducive to high wear, while it would seem plain that chilled flanges must act as admirable shears to trim off "flowed" edges.

Dr. Dudley says low phosphorus units ought to give the best wear, and so they ought. He proceeds to prove it by grouping the 32 rails of least wear, and the 32 rails of most wear in two groups, and taking a mean of their analyses. By a happy accident this mean sustains his view, or perhaps it would be fairer to say, from this accident he draws his conclusions. These groups consist of all sorts of rails, of different make, different conditions of wear and different tonnages. Would it not be fairer to compare rails from the same part of the track, subjected to the same conditions of wear, of the same tonnage, and presumably of the same make? Arranging the 64 rails under consideration in this way by means of the history given, I find 30 groups, consisting of twos, threes, and fours. Comparing the phosphorus units and wear per million tons as given in the table, I find that in twelve groups the softer rails have shown the least wear, while in eighteen groups the harder rails show the least wear.

This gives 18 against Dr. Dudley to 12 for him, and it seems as if this mode of comparison were the fairer, if the presumption is correct, that the rails of these different groups were of the same make in each group, because in

such case the chances are that the physical conditions, due to modes of manipulation in manufacture, were more nearly the same than they could possibly be in the more general average given in the table.

I would like to ask Dr. Dudley to relieve us, if he can, of a term which he has introduced into his papers and specifications, and which, for the sake of harmony, ought, I think, to be obliterated. I refer to the word "hardener." This is a term properly applied to any substance contained in steel, for anything mixed with iron will make it harder. But carbon is the great "hardener," and produces all of the wonderful and useful properties in steel with which we are familiar, and of the nature of which we know so little. Carbon, then, ought to to be distinguished as *the* hardener, and all other components should be known by some other name. If not, we shall have quack steelmakers coming out with more silicon steel, phosphorus steel, sulphur steel, and the like. And why not? Are they not all hardeners, and is any one better than another?

This is a serious matter, for we have too much duplication of meanings now. For instance, if you go to a founder and ask if he has a chill to make a certain size of die or roll, he asks, in reply, how much chill you want, and you tell him, say, half an inch or an inch, as the case may be. Then he asks if you want a tough chill, or a mild chill, or a hard chill. Would not an outsider be utterly puzzled to know what a chill was?

Again, a steelmaker talks of temper, and refers to steel of 0·30, 0·40, or 1·00 carbon, as it may happen; and a steel-user talks of temper, and means a yellow, or brown, or blue color, left on his steel after it is tempered. I once traveled many hundreds of miles to see about a steel trouble. A buyer had sent back a shear-knife which would not cut. Not waiting till the knife was received, I started off, and asked my partner to telegraph me his opinion after he had received the blade. We were both sure it had been burned. After my arrival on the scene and finding a man in real trouble, and a great temper, a message came in these words, "Temper too high; will send another bar." Greatly pleased, and thinking my way plain, in an evil moment I showed the message to my troubled friend, who interpreted it to mean that he had improperly treated the steel, and the indignation created was as profound as it was unexpected.

Who, then, can define steel? An International Committee of this Institute and many others have wrestled with the question, and yet

to-day there is a heavy suit pending in the United States courts, all turning upon the question whether steel is steel or iron.

Now we are threatened with the war of the "hardeners," and the contemplation of another complication in our nomenclature is no joke for the steel makers; and in their behalf I appeal to Dr. Dudley to relieve us before it is too late.

In conclusion, allow me to say that I hope my remarks will not be regarded as antagonistic to Dr. Dudley's great work, for I could have no possible reason for entering the lists as an antagonist; and, on the contrary, I decidedly agree with him that the chemistry of steel is an important factor in its manufacture, and chemistry and physics must go together if results of any value are to be reached; and I firmly believe that eventually all engineers will know how to make and to appreciate chemical specifications. I therefore feel that we should all contribute all we know or think on the subject, and I hope these remarks will be considered a thoroughly sympathetic criticism.

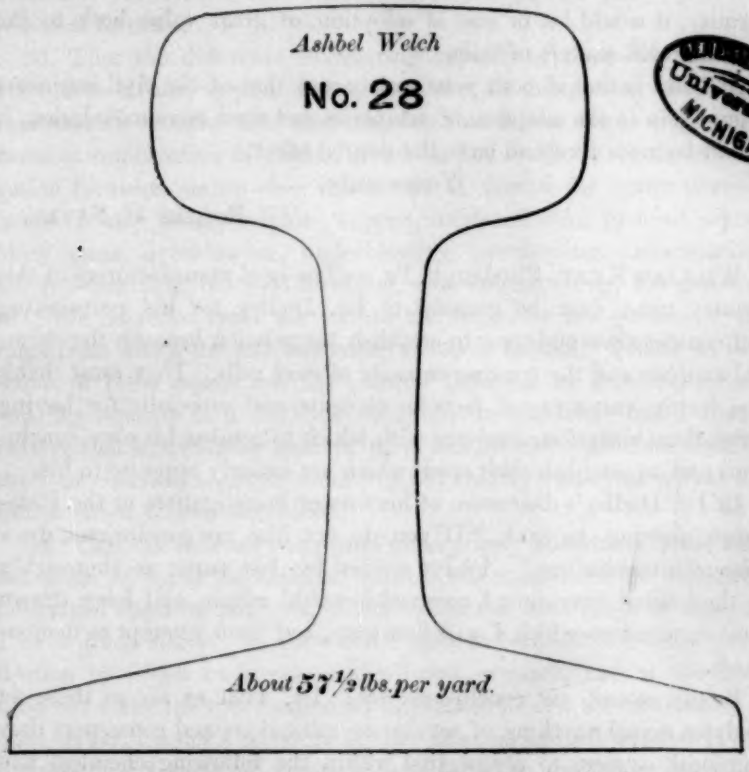
Letter from William R. Hart.

I was this morning an interested listener to the remarks of Mr. Ashbel Welch in regard to his designing a new section for steel rails, in 1866; and for the sake of the truth of history, and in order to give the credit to an American (and where it rightfully belongs) of designing the first section for steel rails, which was intelligently adapted to that material, I beg to state the following facts:

On the 14th of August, 1866, Mr. Ashbel Welch gave me an order for 200 tons of steel rails, to be made for the Camden and Amboy railroad, from a section designed by him. A copy of this section I append herewith.

Messrs. John Brown & Co., of Sheffield, for whom we were then agents, at first declined to roll these rails, owing to the thinness of the flange, but subsequently accepted this order. In principle, as I think you will see from the section, this pattern was similar to what is now known as the Sandberg section, having a large amount of metal in the head, and without superfluous weight in the stem and base. This section was put upon our book, with the title "Ashbel Welch section;" and this name was also rolled upon the stem of the rails. We sold afterward large quantities of these rails under this title to the Philadelphia and Baltimore Railroad Company, as well as to the Camden and Amboy Railroad Company.

I make this statement in justice to Mr. Welch, who ought to have the credit of designing a section which had much to do with making steel rails a success. At the time Mr. Welch designed this section it



University of MICHIGAN

was quite a new departure, and as our old section-book shows it was very different from any of the sections which up to that time had been ordered by any of the railroad companies.

I shall be very glad if this fact can be recorded upon the minutes of the Institute, and remain,

Yours very truly,

WILLIAM R. HART.

PHILADELPHIA, Feb. 17th, 1881. *Agent, Naylor & Co.*

LETTER FROM R. H. SAYRE, BETHLEHEM, PA.

. . . . . The subject is one of great interest in every point of view to railroad managers and steel-rail makers. It has occurred to

me that if in this connection your society would take up the matter of the shape or section of steel rails and the form of joint, and be able to arrive at such form for different weight of rails as you could recommend to the railroads of this country with a view of obtaining uniformity, it would be, in case of adoption, of great value both to the railroads and makers of rails.

My idea is that if both your society and that of the civil engineers should join in the adoption of templates and their recommendation, it would be more likely to have the desired effect.

Yours truly,

ROBERT H. SAYRE.

WILLIAM KENT, Pittsburgh, Pa.:—The steel manufacturers of this country must ever be grateful to Dr. Dudley for his painstaking and conscientious endeavor to establish the relation between the chemical analysis and the wearing capacity of steel rails. They must thank him for the vast array of facts he presents, and especially for having given them sixty-four analyses with which to combat his own conclusions and to establish their own, which are entirely opposite to his.

In Dr. Dudley's discussion of his former investigation, at the Pittsburgh meeting, he said, "If you do not like my conclusions, draw your own conclusions." I have studied his last paper as thoroughly as the limited time since I received it would admit, and have drawn some conclusions which I will first state, and then attempt to demonstrate.

Briefly stated, my conclusions are: 1st, That as far as these 64 analyses reveal anything of service to railmakers and consumers they do reveal, or seem to reveal, that within the following chemical and physical limits, viz.:

Carbon,	0·20 to 0·60
Phosphorus,	0·026 to 0·145
Silicon,	0·015 to 0·480
Manganese,	0·252 to 0·880
Phosphorus units,	20·3 to 5·72
Bending weight,	2270 lbs. to 4260 lbs.
Deflection,	13° to 190°

or in other words within the limits of nearly the whole range of the chemical and bending tests of these 64 rails, the wearing capacity bears no relation at all to carbon, to phosphorus, to silicon, to manga-

nese, to phosphorus units, to bending weight or to deflection; or if there is any relation between the wearing capacity and these six or seven variables, it is so obscured by the action of other causes or variables not yet known, that such relation cannot be expressed by any practical formula.

2d. That the difference in wearing capacity of these 64 rails was not due to carbon, to phosphorus, to silicon, to manganese, or to any combination of these four elements, but that it was due to some other cause or combination of causes, of which Dr. Dudley's whole investigation furnishes us no clue whatever. A few of the many possible causes I may name, sulphur, copper, oxide of iron, inclosed air or other gases, overblowing, underblowing, overheating, underheating, too hot-finishing, too cold-finishing, cold-straightening, too great or too little reduction from the rail to the ingot, or the portion of the ingot from which the rail was taken, as top or bottom. I have no idea which of these causes has the greatest influence in determining the wearing capacity of a rail,—probably no one else has,—but I firmly believe that some one or more of them has far more influence than all the four chemical elements named in Dr. Dudley's analyses within the limits which I have mentioned.

3d. That the railroad companies must utterly abandon, for the next ten years at least, the attempt to limit rail manufacturers to certain prescribed chemical analyses, unless within the wide range of analyses I have given above. If they would seek to establish a definite specification by which to insure good wearing capacity, and at the same time not make the specification an impracticable one for railmakers to meet, as Dr. Dudley's certainly is, they must inaugurate another investigation (and I know of no one so well fitted to undertake it as Dr. Dudley himself, after he shall have thoroughly disabused his mind of conclusions already formed), which investigation shall take at least ten years to complete, and shall include not only the effect of the four chemical elements now under discussion, but all the other supposed causes of difference in wearing capacity which I have mentioned above, besides many others I have not even thought of. Such an investigation I believe the railmakers would not object to; they should rather contribute towards it. It would richly repay the railroad companies by enabling them to secure better rails, providing that after the investigation was concluded it should reveal the causes of defective wearing capacity, which Dr. Dudley's present investigation does not do.

4th. That Dr. Dudley's failure to establish, after years of careful investigation, the relation between the chemical analysis and the wearing capacity of a rail, should be a lesson to other consumers of steel besides the railroad companies, not to attempt to regulate the steel manufacturer by a chemical specification based upon their own investigations, far less elaborate than that of Dr. Dudley or that of the manufacturers. It is a common occurrence for a steel manufacturer to be asked to guarantee both a chemical and a physical specification entirely inconsistent with each other, and one or both impossible of fulfilment. The attempt to fill orders with such specifications is not only annoying but often even ruinous to the manufacturer, causing the rejection of much steel that is even better adapted to the wants of the consumer than some of the steel which is accepted. This is precisely the result which would happen if Dr. Dudley's rail specifications were to be insisted on.

I will now undertake to give a reason for the conclusions I have drawn. Suppose that the Pennsylvania Railroad Company were at once to insist that all rails furnished them should conform to Dr. Dudley's specifications in these six particulars:

Carbon, ...	·25 to ·35
Phosphorus, ...	not over ·10
Silicon, ...	not over ·04
Manganese, . . .	·30 to ·40
Bending weight, ...	not over 3000 lbs.
Deflection, . . .	not under 130°

Suppose that before the rail manufacturers had "got the hang" of making rails within these rigid limits, and while they were yet making them with the wide range of composition which they are now doing, I should be sent as inspector to one or more works to inspect 64 lots of rails, one rail being taken out of each lot for test, the test rail being supposed to represent exactly the lot from which it was taken. Suppose, further, that I test these 64 rails chemically and physically, and find that the results are exactly those given in Dr. Dudley's tables. I tabulate the results as I have done here, rejecting all the tension and torsion tests, as these are not included in my instructions, and I carefully examine the table to see how many I should accept and how many I should reject. How many of these 64 rails or lots would I have to accept under these specifications? *Only three!* And one of

these three would be classed by Dr. Dudley as a bad rail, standing number 10 out of the 16 rails marked "Level Tangent."

I would reject *sixty-one* of these sixty-four rails (or lots) for violation of from one to all six of the specifications.

34 for too great bending load.
29 " small deflection.
42 " high or too low carbon.
31 " high phosphorus.
32 " high silicon.
52 " high or too low manganese.

As Dr. Dudley classes the 64 rails into two grand divisions, viz., 32 slower wearing and 32 faster wearing (I call them here good rails and bad rails for the sake of brevity), I have to reject 30 of his *good rails.*

13 for too great bending load.
7 " small deflection.
21 " high or too low carbon.
11 " high phosphorus.
14 " high silicon.
21 " high or too low manganese.

On the tables (Plates 6 and 7)* I have marked each cause of rejection with a black mark. You will notice that they seem covered all over with black marks, that their position follows no regular law, but that I seem to have distributed them with commendable impartiality. I count the black marks, and find the total causes of rejection to number 210 out of a possible 366, or an average of 3·44 for each of the 61 rails rejected. Of the 31 bad rails rejected the black marks or causes of rejection number 123, an average of 3·97. Of the 30 good rails rejected the causes number 87, an average of 2·9. Here is a point in favor of Dr. Dudley: the 31 bad rails rejected do have a worse chemical and physical record than the 30 good rails rejected, the average number of black marks against the bad rails being greater in the ratio of 3·97 to 2·9. But his gratification in this regard must be somewhat diminished when he learns that by selecting a number of very, *very* bad rails, which I have done on the tables, drawing heavy lines around them, and marking them *worst rails*, viz., the two worst rails out of 16 marked tangent grade, the four worst rails out of 8 marked curve

* See JOURNAL for March, 1881.

grade, low side; the three worst out of 8 curve grade, high side; the three worst out of 16 level tangent, the two worst out of 8 level curve, low side, and the four worst out of 8 level curve, high side, the total causes of rejection of these 18 rails is 68, or an average of only 3·78.* That is, the 18 worst rails have fewer average causes of rejection than the whole 31 rejected rails designated "faster wearing" in the ratio of 3·78 to 3·97.

In Dr. Dudley's table of averages he compares the chemical and physical properties of 32 slower-wearing rails with those of 32 faster-wearing rails in all conditions of service. The comparison agrees with his conclusion, except in the matter of silicon, the average silicon of the bad rails being lower than that of the good rails. Let us carry the comparison a little further, and compare the properties of the 18 worst rails, which I call very, *very* bad rails, with those of Dr. Dudley's 32 bad rails, and see whether it strengthens his position. Here is the comparison:

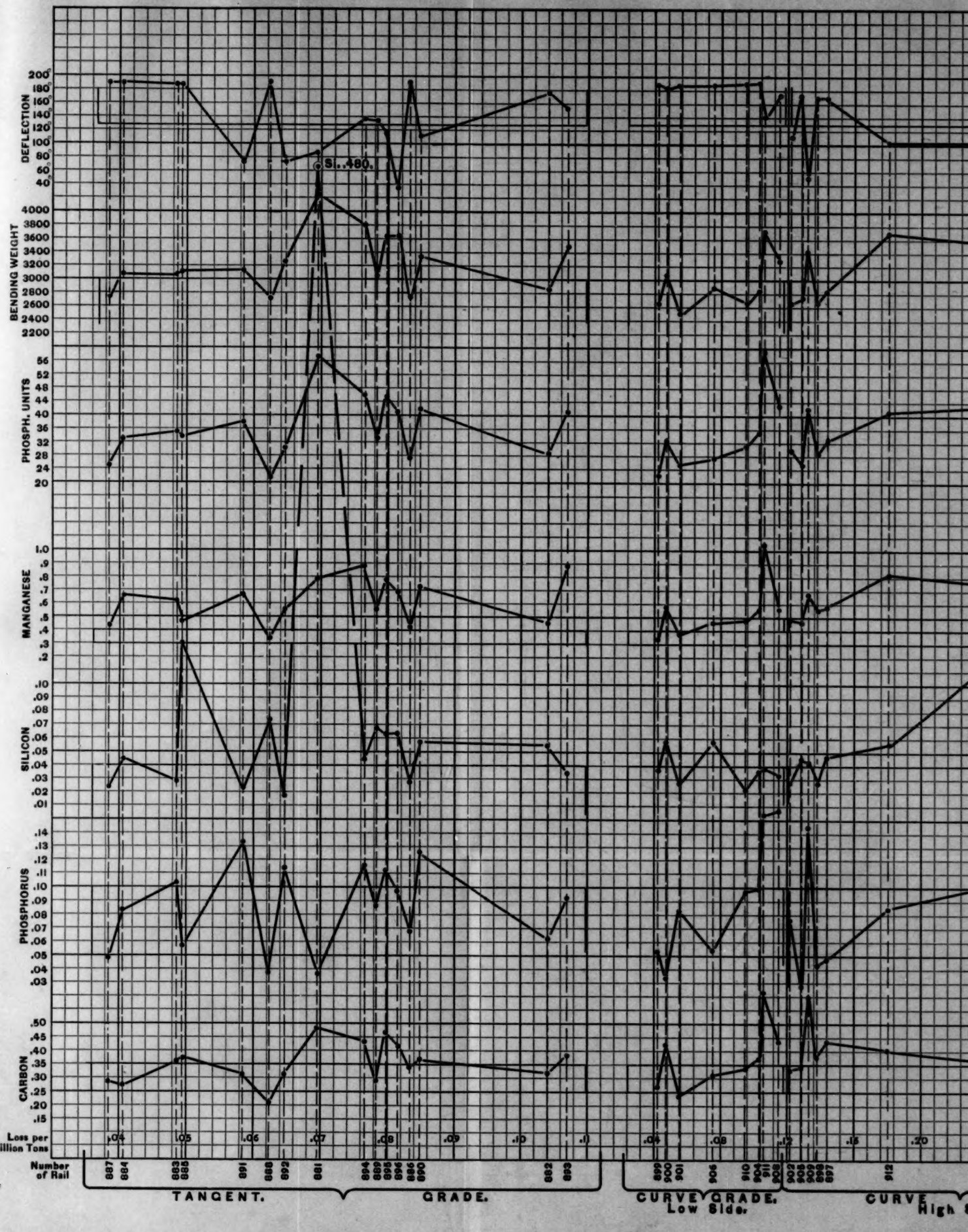
	Loss per million tons.	Bending weight.	Deflection.	C.	P.	Si.	Mn.	Phos. units.
32 good rails,	·0506	2878	160°	·334	·077	·060	·491	31·3
32 bad rails,	·1028	3222	133°	·390	·106	·047	·647	38·9
18 worst rails,	·1326	3209	135°	·412	·109	·040	·677	40·4
13 bad (?) rails,	·0668	3267	136°	·369	·105	·050	·632	37·9

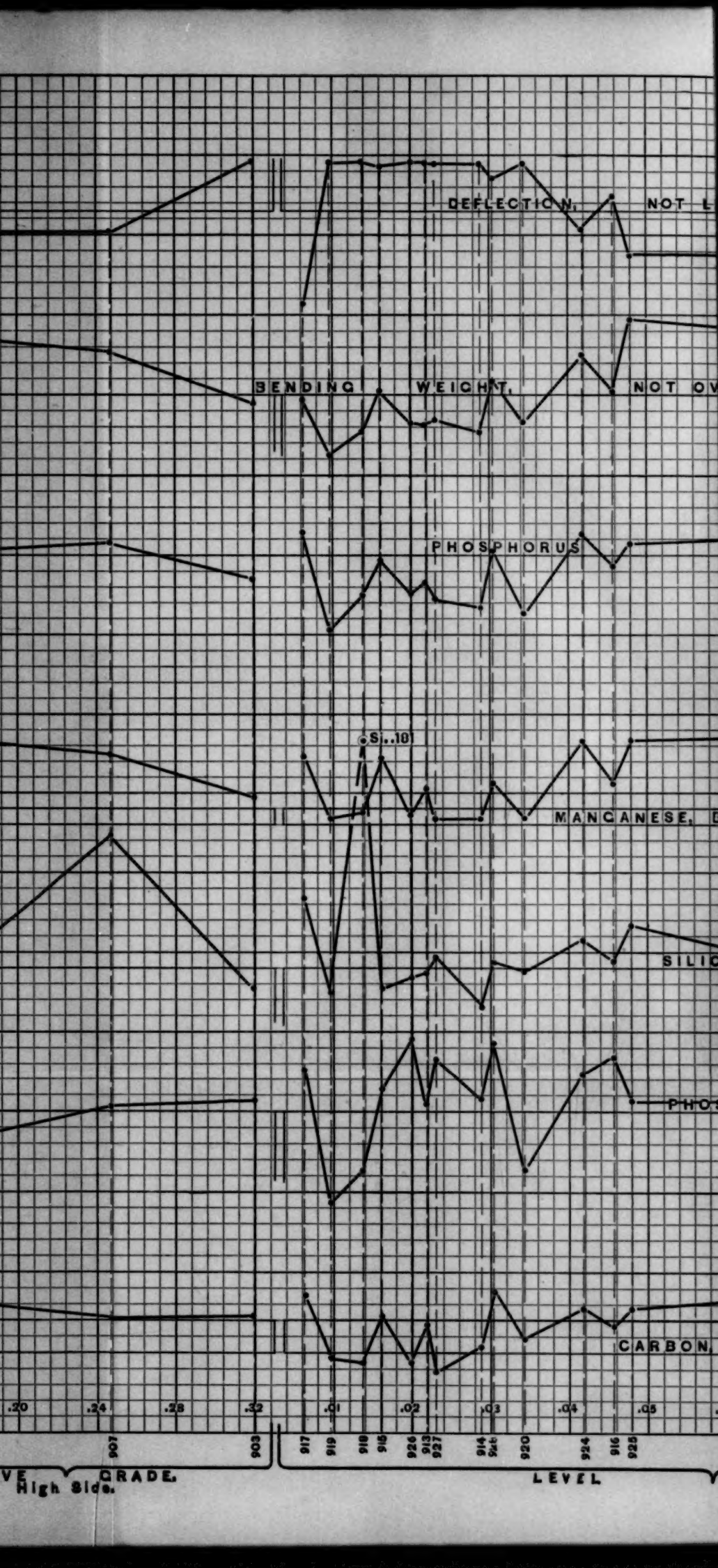
The 18 worst rails show a greater loss per million tons than the 32 bad rails by 29 per cent., while in three elements of Dr. Dudley's specifications, viz., bending weight, deflection and silicon, they are slightly better, according to his ideas; and in three others, viz., carbon, phosphorus and manganese, slightly worse. If Dr. Dudley's conclusions were correct, we should expect to find that a lot of rails having 29 per cent. worse-wearing capacity than a larger lot which included them, would show a very marked deviation in average analysis and bending tests from the average analysis and bending tests of the larger lot; but actually the deviation is so slight that Dr. Dudley himself could not tell, if the results of tests alone were presented to him, which lot had the greater and which the less wearing capacity.

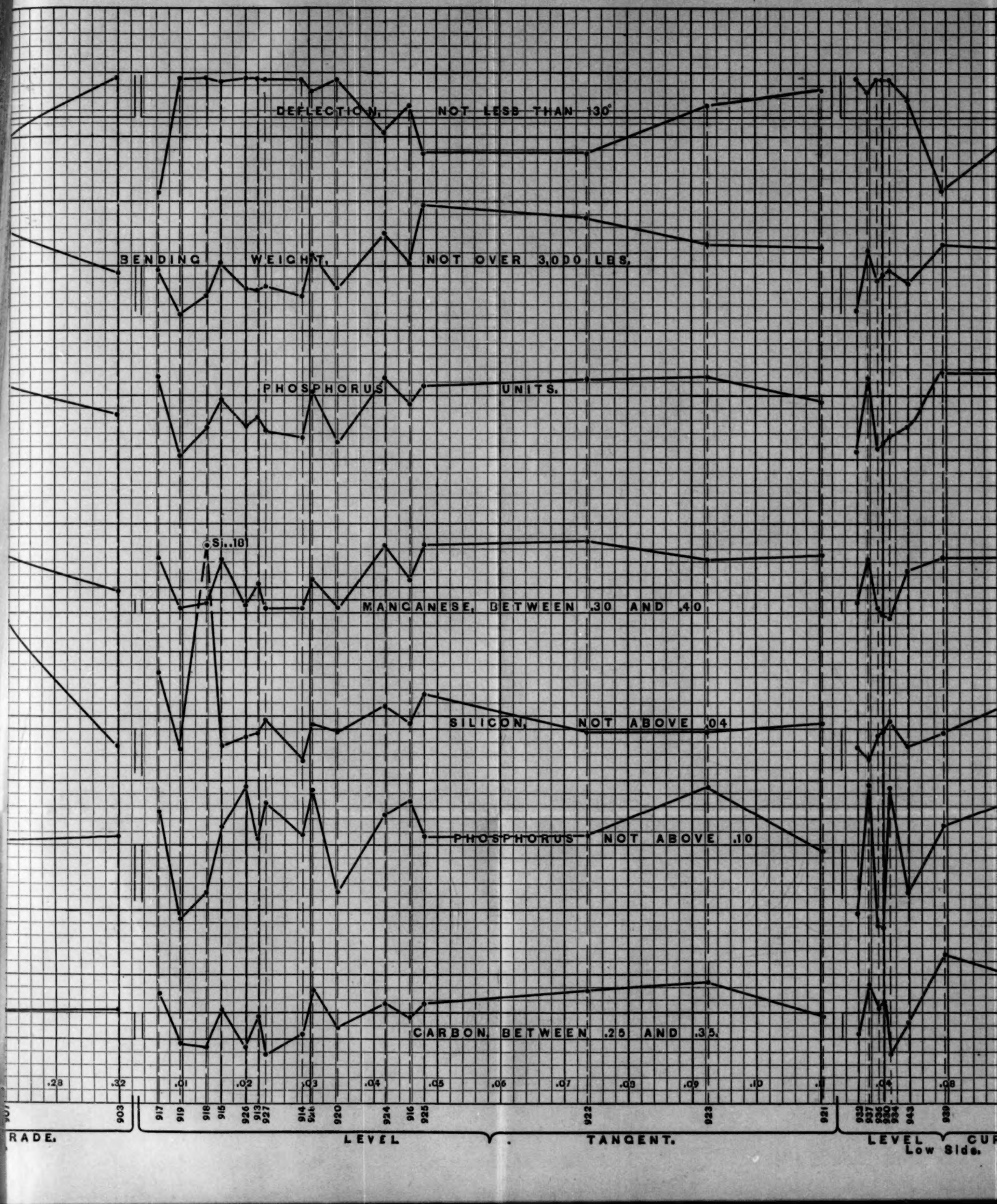
But I have carried the comparison still further. In the 4th line in the table I have placed the record of the other 13 bad (?) rails, out of

* The method of selection of these 18 rails is not an arbitrary one, as will be seen further on. Of the 16 level tangent rails, 14 should be classed as good and 2 as bad, a fairer classification than that of 8 faster and 8 slower wearing.

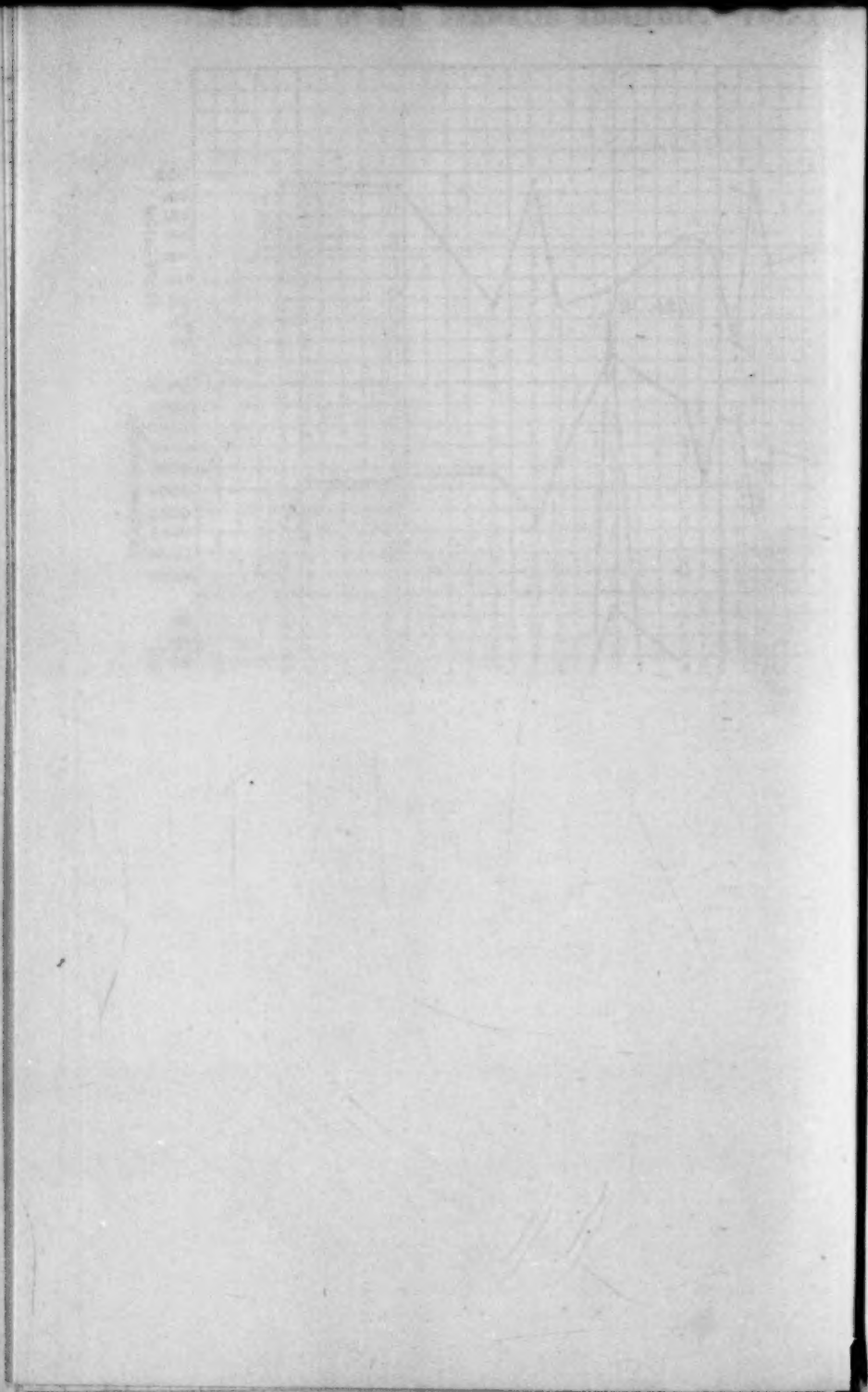
Grant.—Plate 1.


DEFLECTION
200
180
160
140
120
100
80
60
40
Si. .480.
BENDING WEIGHT
4000
3800
3600
3400
3200
3000
2800
2600
2400
2200
PHOSPH. UNITS
56
52
48
44
40
36
32
28
24
20
MANGANESE
1.0
.9
.8
.7
.6
.5
.4
.3
.2
SILICON
.10
.09
.08
.07
.06
.05
.04
.03
.02
.01
PHOSPHORUS
.14
.13
.12
.11
.10
.09
.08
.07
.06
.05
.04
.03
CARBON
.50
.45
.40
.35
.30
.25
.20
.15
Loss per Million Tons
.04
.05
.06
.07
.08
.09
.10
.11
.04
.08
.12
.15
.20
Number of Rail
887 884 883 888 891 888 892 881 894 889 895 896 890 882 893
899 900 901 906 910 904 911 908 902 905 909 898 897 912
TANGENT.
GRADE.
CURVE
GRADE.
Low Side.
CURVE
High


DEFLECTION, NOT L
BENDING WEIGHT, NOT OV
PHOSPHORUS
S...18
MANGANESE, B
SILIC
PHOS
CARBON
.20
.24
.28
.32
.01
.02
.03
.04
.05
907
903
917
919
918
915
926
913
927
914
920
924
916
925
VE GRADE.
High Side.
LEVEL


DEFLECTION, NOT LESS THAN 130°
BENDING WEIGHT, NOT OVER 3,000 LBS.
PHOSPHORUS UNITS.
Si..18
MANGANESE, BETWEEN .30 AND .40
SILICON, NOT ABOVE .04
PHOSPHORUS NOT ABOVE .10
CARBON, BETWEEN .25 AND .35.
.28
.32
.01
.02
.03
.04
.05
.06
.07
.08
.09
.10
.04
.08
903
917
919
918
915
926
913
927
914
920
924
916
925
922
923
921
933
937
930
934
943
939
RADE.
LEVEL
TANGENT.
LEVEL
Low Side.

.08
.12
.16
.04
.08
.12
.16
CURVE
Side.
LEVEL
CURVE
High Side.



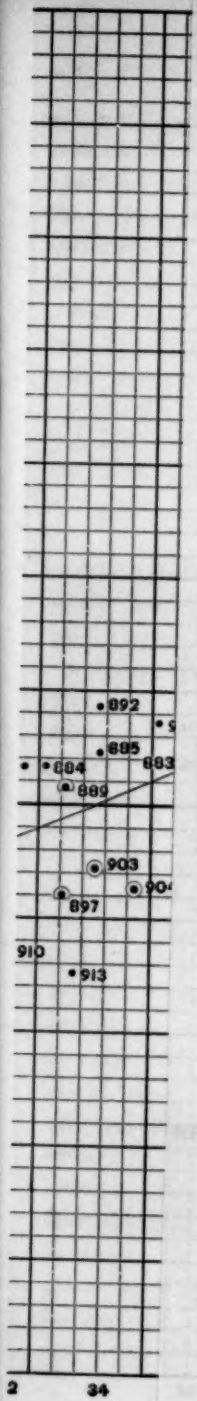
the 31 faster-wearing rails, which would have been rejected under Dr. Dudley's specifications. (I have omitted in this average rail No. 920, classed by Dr. Dudley as a bad rail from its faster wearing, but which conforms to his specifications in every respect.) I have placed an "?" after the word bad in designating these rails, as I think they should have been classed with the 32 good rails, on account of their wearing capacity being much nearer to that of the good rails than to that of the 18 worst rails with which they are associated. Comparing the 18 worst rails with the 13 bad (?) rails, we find the former to be over 98 per cent. (or nearly double) worse in wearing capacity than the former, while their analyses and bending-tests indicate them to be very nearly alike. The worst rails are lower in bending weight and silicon, and only slightly lower in deflection, and slightly higher in carbon, phosphorus and manganese. Here is a very plain case selected from Dr. Dudley's own work. Two distinct lots of rails, one lot twice as good in wearing capacity as another, but both lots closely agreeing in average analysis and bending tests. What stronger evidence could be produced of the fallacy of his formula?

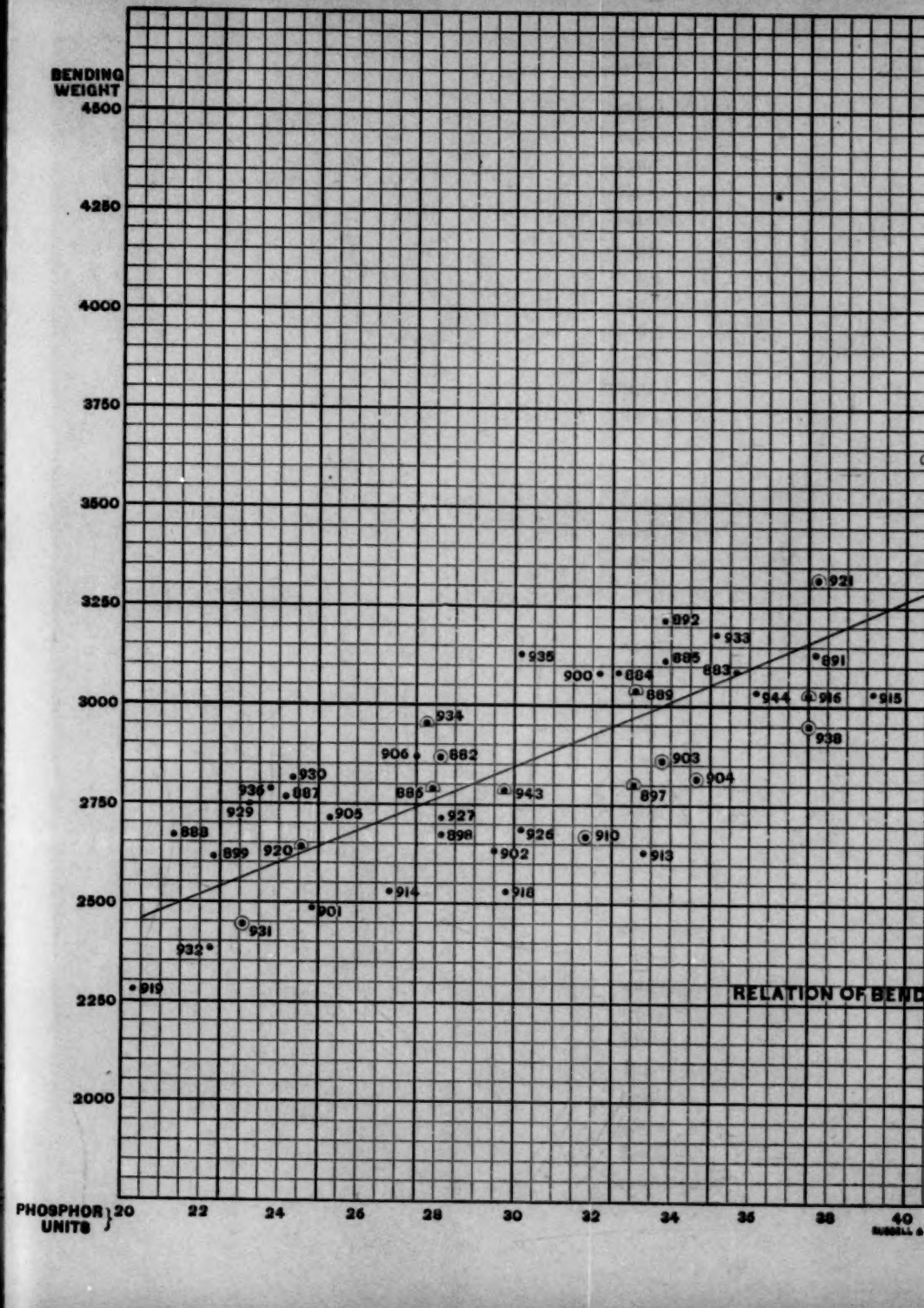
If, in any large series of observations of the values of different variable quantities, such as are here dealt with, there exists any law of interdependence of such variables, the figures representing such observations can be plotted on cross-section paper, and the law will be plainly revealed by a curve or straight line drawn through the various observations. I have thus plotted the results of Dr. Dudley, and have made 42 curves, or attempts at curves, to discover, if possible, the existence of any law of interdependence of the variable wearing capacity in the six series of rails, viz., tangent grade, curve grade high side, curve grade low side, tangent level, level curve, high side and level curve low side, with the seven other variables; bending weight, deflection, carbon, silicon, phosphorus, manganese and phosphorus units. Here is the diagram (Plate I) with the 42 attempts to form as many curves. You will see there is neither curve nor straight line here—nothing but a heterogeneous mass of ups and downs and straights across—not the slightest indication of any law in any single curve which is not contradicted by another curve of the same variable in another series.

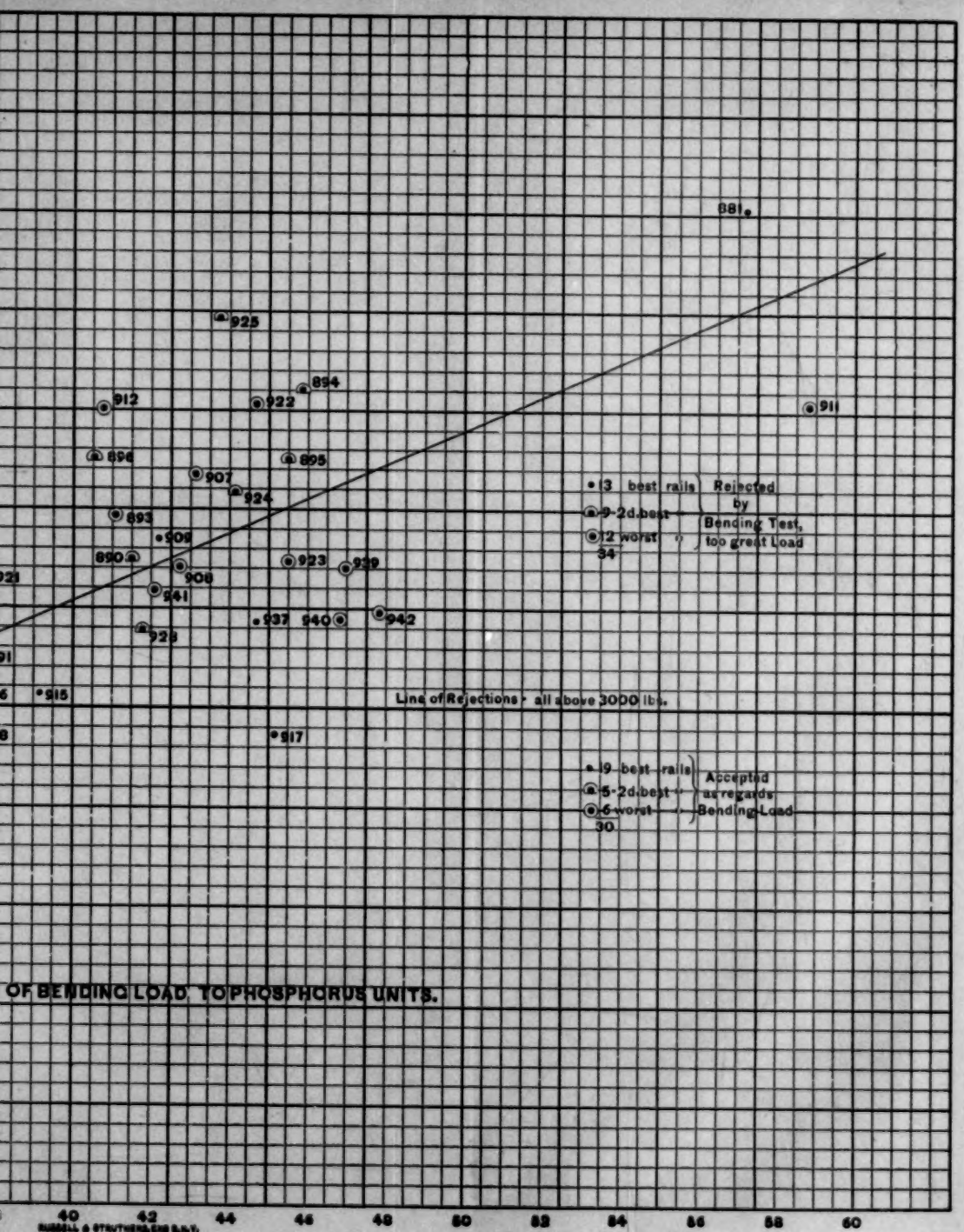
This set of curves, or rather zigzags, shows plainly my reason for separating Dr. Dudley's 32 bad rails into two series, one of 18 worst rails, the other of 14 which nearly approached the good rails. In the

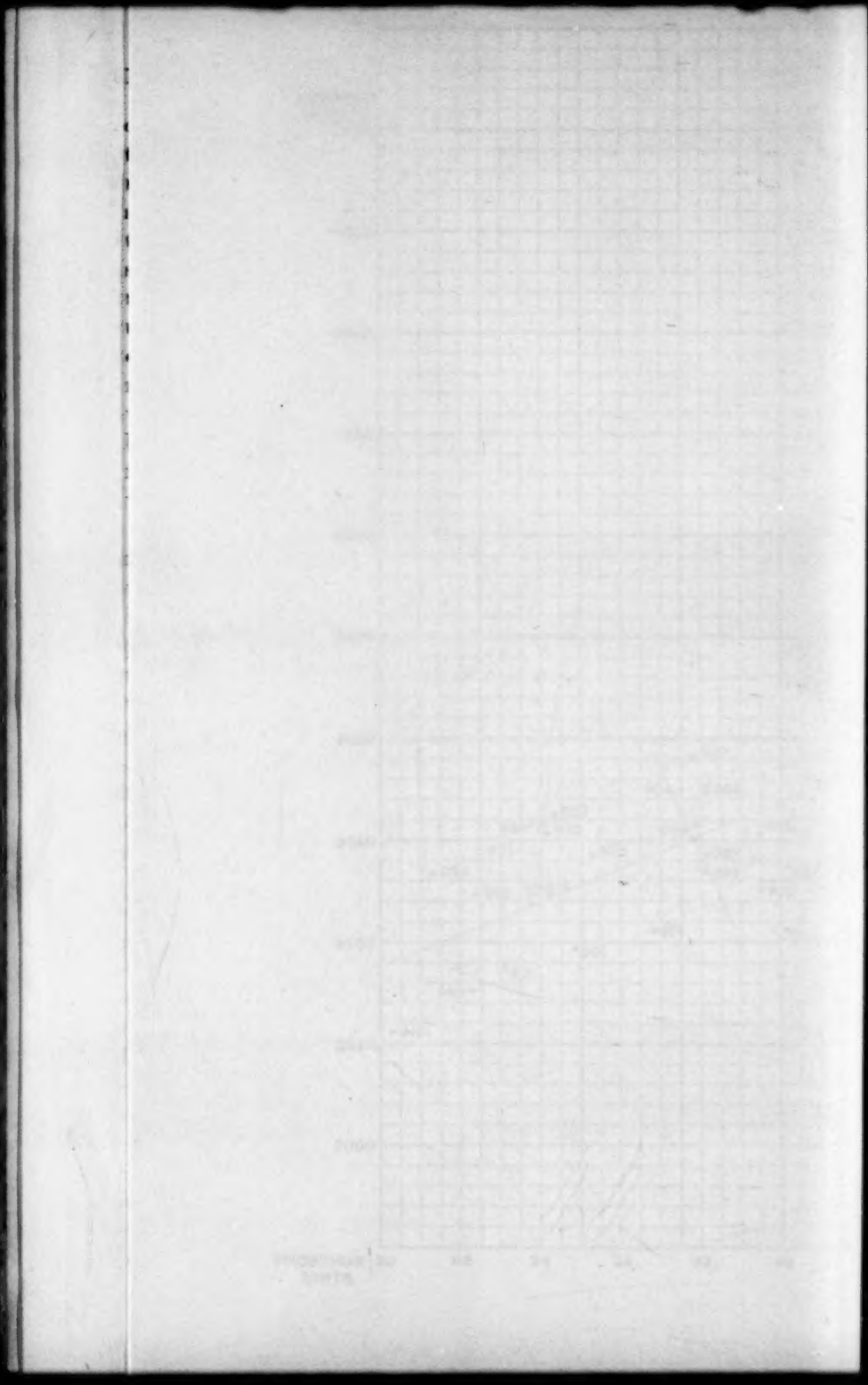
curves of the tangent grade rails the two worst rails, 882 and 893, in their plotted positions in the curve remove themselves far from the main body of these rails, and therefore in contrast with the others they are justly named the very, *very* bad rails of this lot. This grouping of the very bad rails far away from the better rails is still more plainly seen in the level tangent rails, where 13 rails are comparatively near together in position, and the other three are far removed from them, thus plainly indicating that if we wish to consider the relation of the chemical composition of these 16 rails to their wearing capacity, the method of averaging the 8 faster-wearing and the 8 slower-wearing—which Dr. Dudley seems to think is the only good one—is not the best by any means, but that to obtain an intelligent deduction the 13 fairly good rails must be contrasted with the three rails which are widely different from them in wearing capacity.

I have said that these zigzags indicate the absence of any law of relation between wearing capacity and the six variable quantities under consideration. I am surprised that Dr. Dudley did not include his phosphorus units in his specification, as I regard this idea of phosphorus units as a most valuable conception, and one likely to lead to the advancement of our knowledge of the relation of the physical to the chemical qualities of steel. For this reason I have included phosphorus units in my plate of zigzags, although Dr. Dudley has not named it in his specifications. I thought it probable that by plotting the phosphorus units, I might possibly discover whether they had any relation to wearing capacity, although carbon, phosphorus, silicon, and manganese had no such relation. I discovered none. But there is a very marked peculiarity in the zigzags representing phosphorus units besides that of general relation to the four chemical elements which follow as a matter of necessity, namely, the almost entire parallelism of the lines of phosphorus units and bending weight. This suggests at once the existence of a relation between phosphorus units and bending weight, and I have, therefore, made another diagram, plotting the figures expressing this relation. The accompanying Plate II shows bending weight at the side of the section paper, and phosphorus units at the bottom. You will plainly notice that there is an evident trend of the dots representing the rails in the direction of an ascending inclined straight line. The whole range of bending weights being 2000 pounds, the deviation of the position of any rail from this inclined straight line (with only two exceptions) is an ordinate representing less


892
895
884
883
889
903
897
910
913
2
34


BENDING WEIGHT
4500
4250
4000
3750
3500
3250
3000
2750
2500
2250
2000
PHOSPHOR UNITS
20
22
24
26
28
30
32
34
36
38
40
RELATION OF BEND
919
932
931
901
914
916
902
926
910
913
898
927
905
920
899
888
929
936
887
930
885
943
897
904
906
882
934
938
903
915
916
944
889
883
884
900
935
892
933
891
921


881.
925
894
912
922
911
898
895
907
924
893
909
890
923
939
908
921
941
937
940
942
923
915
Line of Rejections - all above 3000 lbs.
917
13 best rails
9 2d best
12 worst
34
Rejected by Bending Test, too great Load
19 best rails
5 2d best
6 worst
30
Accepted as regards Bending Load
OF BENDING LOAD TO PHOSPHORUS UNITS.
40
42
44
46
48
50
52
54
56
58
60
RUSSELL & STRUTCORE, ENG S.N.Y.



than 450 pounds. The inclined straight line is the average direction of trend of these dots, and the individual dots do not deviate greatly from this average direction. The law of the relation between phosphorus units and bending weight is thus clearly established, and it is this: *The increase of bending weight is directly proportional to the increase of phosphorus units.* The discovery of the law is a strong presumptive proof of the correctness of Dr. Dudley's hypothesis in regard to the hardening influence of the four chemical elements upon steel, viz., that the relations of phosphorus, silicon, carbon and manganese are to each other in respect to hardening as the numbers 1, $\frac{1}{2}$, $\frac{1}{3}$ and $\frac{1}{5}$. Dr. Dudley is certainly to be congratulated upon this discovery of presumptive proof of his hypothesis.

The fact of the law of relation of bending weight to phosphorus units being so plainly indicated in these 64 analyses and tests, while there is no such indication from these tests of any similar relation existing between wearing capacity and phosphorus units, or between wearing capacity and carbon, phosphorus, silicon, manganese, bending weight or deflection, is almost absolute proof in itself that such relation does not exist at all, or, as stated in the first part of my remarks, if it does exist it is entirely obscured by the influence of other variable quantities not considered in Dr. Dudley's paper.

On the diagram on which is plotted the relation of phosphorus units to bending weight I have indicated the position of each of Dr. Dudley's 32 slower-wearing rails by a single dot. The 18 worst rails of the 64 are indicated by a dot surrounded by a circle, and the remaining 14, called second-best on the diagram, are indicated by a dot surrounded by a semicircle. By drawing the line between rails which would be rejected on Dr. Dudley's specification for bending weight alone, viz., not above 3000 pounds, and counting those above the line, it is seen that there are 34 rails rejected out of the 64, and of these 34, 13 are the best rails of the series (Dr. Dudley's slower-wearing rails), 9 are the second-best rails,—almost as good as the former,—and 12 are from the 18 worst rails. Counting the accepted rails, a total of 30, 19 are best rails, 5 are second-best, and 6 worst. As already shown, of these 30 rails, which might be accepted on the ground of their bending weight being below 3000 pounds, only three of them could pass the gauntlet of all the other five specifications, and one of these three is a second-best rail.

I think I have now proved my first proposition, but I must antici–

pate an objection which Dr. Dudley may possibly raise. He may say that my argument against his formula, based upon the rejection of 61 rails out of 64, is invalid, because these rails were not made according to his formula. Let them be made according to my formula, he may say, and they will not be rejected, and they will give good service. I regret that he has given us the record of only three rails which do conform to his formula; they are not sufficient in number to draw valid conclusions from, but, such as they are, here is the conclusion they lead to. Two of them are slower-wearing, one a faster-wearing rail; therefore, out of three rails which conform to the formula, the chances are that one rail will not be a good one. Are the railroad companies satisfied with such a result as this? But suppose, for the purpose of admitting a greater number of rails into our accepted list, we relax the specifications slightly. We will say that rails must be made according to Dr. Dudley's formula, but if a rail happens to conform to five of the six specifications and fails in only one, and that one manganese, the least injurious element considered (Dr. Dudley himself considering it only one-fifth as bad as phosphorus), then we will not reject that rail. Our list of accepted rails will then read as follows:

No.	Service.	Order in Dr. D.'s table.	Max. load.	Deflection.	C.	P.	Si.	Mn.	P. units.
920	Level tangent,	10th in 16	2630	190°	·293	·063	·039	·326	24·6
932	" curve, low side,	1st " 8	2340	190°	·269	·047	·026	·372	22·4
943	" " "	6th " 8	2780	159½°	·314	·061	·025	·602	29·8
931	" " high side,	5th " 8	2430	190°	·260	·047	·029	·416	23·1
887	Tangent grade,	1st " 16	2770	190°	·287	·048	·023	·435	24·2
886	" "	13th " 16	2790	190°	·349	·069	·024	·404	27·9
899	Curve gr., low side,	1st " 8	2620	190°	·263	·051	·038	·326	22·3
910	" " "	5th " 8	2660	190°	·343	·098	·020	·478	31·8

Here are eight rails, unquestionably the softest rails of the series, conforming exactly to Dr. Dudley's specifications, except four which have manganese less than one-tenth of a per cent. higher, and one with manganese two-tenths of a per cent. higher than the specifications. We ought to expect that all or nearly all of these eight rails would be included in the thirty-two rails designated by Dr. Dudley as slower-wearing, but, on the contrary, there are only three of them so included, and five of them are the faster-wearing rails. It has been said that exceptions prove the rule, but there is such a thing as having more exceptions than rule, and I think we have such a case before us.

After Dr. Dudley has spent some years investigating the difficult problem of determining what physical or chemical properties have an influence upon wear, he tabulates his results; he studies them carefully; he then proceeds to write his report and to draw his conclusions. In the report we find the following words:

"Giving our attention now to the tables, I think the first observation will be that there is no absolute gradation in physical qualities, or in chemical composition, applying to every rail in each group, which corresponds to the gradation in amount of metal lost per million tons."

Then he tells us we ought not to expect such uniformity, for two reasons:

1st. Errors in determining loss of metal and tonnage.

2d. I quote his own words: "I am not aware that it is known as yet exactly what wear is, or what it is dependent upon; . . . . . whether wear is a direct function of the tensile strength of steel, or of its elongation, or of its elastic limit, or of its resilience, or any combination of these, or indeed, as seems somewhat probable, of the amount of distortion by bending that a piece of steel will suffer, is a problem yet to be solved."

It certainly is a problem *yet to be solved*, and it will take many years to solve it. Dr. Dudley should have stopped here, and drawn the conclusion which I have drawn, namely, that, within the wide range of analyses and bending tests which I mentioned in the first part of these remarks, as far as these sixty-four tests show anything, they indicate that whatever wear is or may be dependent upon it is not dependent upon carbon, phosphorus, silicon, manganese, bending weight and deflection. Instead of this, in one breath he admits he does not know what wear is dependent upon, and in the next he formulates the extraordinary *non sequitur* that it is dependent upon carbon, phosphorus, silicon, manganese, bending weight and deflection, and recommends that the Pennsylvania Railroad Company demand that rails be made on specifications, based on these six variables, so narrow that to fill them would cause the constant rejection of enormous quantities of steel, and a consequent enhancement of the price of rails, probably ten or twenty per cent., without any certainty that such rails would be any better than those the steel-mills are now making.

I earnestly hope that the investigation which Dr. Dudley has so ably carried on will be continued. I hope the Pennsylvania Railroad

Company, or preferably a combination of several railroads, will institute the prolonged investigation which I think will be necessary to solve this deep problem; that they will take a hundred or more rails, watching and noting down carefully every detail of their manufacture as well as their analysis; that they will be carefully weighed before, during, and after service; that their crop-ends will be tested before service, and the rails themselves after removal; that all the sources of error which Dr. Dudley admits in the present investigation may be removed, and that enough facts may be gathered and tabulated so that the conclusions which may be drawn from them may be apparent to every one without labored discussion or heated argument. But I venture to prophesy that after this investigation shall have been completed, and a formula adopted which shall be satisfactory to both manufacturers and consumers, that formula will not be the one now under discussion.

I hope Dr. Dudley will pardon me if I have been unduly severe in my criticism, and consider that I have written my remarks hastily and at a time which should have been given to needed rest. I differ with him only as regards the conclusions which he has drawn. I appreciate the value of his labors, and only make public my own conclusions in the hope of contributing to the advancement of the science of steel-making, in which we are all so deeply interested.

Dr. August Wendel, Troy, N. Y.: Dr. Dudley's last paper gives, certainly, very valuable and interesting information regarding the wear of rails under different conditions. His results concerning the composition of rails, explode, rather startlingly, some old theories regarding the wear of rails, and I think after his formula is simplified it will be *one* good formula to work by.

As he now arrives at the same conclusions reached in his first paper, some of my remarks will apply to both, although I would not attempt to add anything directly to the distinguished criticism the first paper received.

Regarding his silicon percentages, I must say I cannot see any reason why they have not been disregarded entirely within the limits of his investigation. In following Dr. Dudley's reasoning in the use of averages and applying inductive methods, I cannot see what importance can be attached to this element, since, in his first investigations, the good and bad rails averaged nearly alike, and in his last series the

rails which wore best, showed even more silicon than the bad ones. I regard, therefore, the silicon an inconsiderable factor in making out the phosphorus units, without considering here the actual influence of this element on hardness,—an influence which is greatly overestimated in the formula.

With regard to the effect of manganese, I cannot agree with his conclusions. In ordinary working with full-blown steel, manganese has more or less the function of carbon, provided the spiegel is constant, and consequently should not be introduced into a formula for daily working as an independent coefficient.

In March, 1875, I made a report to my employers concerning the unsatisfactory working of steel in blooming.* I then came to the conclusion that steel ingots, in order to roll well, ought to contain:

$$Mn = 0{\cdot}8\ (C + \tfrac{1}{2}\ Si) + 4\ P,$$

these symbols standing for the respective percentages of the elements. I maintain to-day, that for good results in blooming, this percentage of manganese ought to be aimed at for rail steel. For this reason I would sooner undertake to make steel according to Dr. Dudley's original formula, viz.:

Carbon,	0·334
Silicon,	0·060
Phosphorus,	0·077
Manganese,	0·491

than according to the one in which he tries to make concessions to the manufacturers, viz.:

Carbon,	0·30
Silicon,	0·04
Phosphorus,	0·10
Manganese,	0·35

Now in spite of all the progress in steel manufacture, we have not succeeded in making up steel by prescription, and what would therefore become of the ingots in which manganese for some cause or other should happen to fall below Dr. Dudley's meagre allowance? The answer will be: "Works must return to the old practice of hammering ingots," but I doubt very much whether even by hammering a sounder bloom, and as a consequence, a better rail is obtained.

German government officials who as a rule are not happy unless

* Transactions, Vol. IV, page 364.

they can make things unpleasant for somebody, must have got hold of Dr. Dudley's formula, as they lately insist upon steel being hammered. Now, there is not the least doubt that some of the steel under Dr. Dudley's investigation was hammered, but I do not deem it necessary to resume that practice, if a larger manganese percentage is used.

I am greatly encouraged in this statement by the analysis of the *rara avis* of the series. I refer to the one showing 0·48 silicon. I would consider it sound metal, since it satisfies my equation regarding manganese, and still, in spite of its increased hardness and its enormous phosphorus units, is satisfactory in wearing capacity.

The conclusions of Dr. Dudley's ingenious experiments seem more simple than he cares to admit, and could be condensed by simply saying: "Use iron low in phosphorus, and do not make the steel too hard."

Regarding tests of steel made from such iron, I would even be more stringent than Mr. Sandberg. Of each blow I would make a bar about one inch square, and bend it cold. It should not be so hard as to resist bending to the shape of a horseshoe, nor should it be so soft as to bend 180 degrees without showing signs of fracture. There would thus be obtained a quality of steel that would more than satisfy Dr. Dudley's pretty theory of infinitesimal teeth by creating those whose tendency would be to neither flatten nor to bend. I think that in making this test, and supplementing it by the carbon test, manganese and silicon would regulate themselves much more nicely than any specification could effect. Every steel maker knows that, should the silicon run high, the heat is blown too short, carbon will be increased considerably, and the test will not stand. On the other hand, if manganese is high, the heat has received too much spiegel (and that is simple awkwardness) and carbon would show the same result as above.

In conclusion, I should think that railroad authorities, under all circumstances, would prefer the steel with which they are now familiar, to a specimen that Mr. Sandberg has described as having broken into seventeen pieces under the wheels. After blowing such low manganese steel, it may be coaxed into a rail, and it is a wonder that it holds together so long as it does, with so great a number of minute flaws.

I would not in any way depreciate chemistry, but I think it should be kept in its proper sphere. Let the chemist look after the quality of pig metal, and apply common sense in the avoiding of extremes; then the most fastidious railroad cannot find fault with the result.

PROFESSOR EGLESTON, New York City: It is not my place as an engineer to apologize for the chemists, but as no one seems disposed to do so, and as they have had more than their share of criticism, I am glad to say that I believe there are chemists in this Institute whose work and word is just as reliable, and perhaps even more so, than that of the average engineer. But we ought to make a distinction; there are chemists and chemists. With the ordinary commercial chemist, who looks upon the science as so much merchandise, I have not a particle of sympathy; but with the chemist who looks upon his profession as engineers do upon theirs, I have every sympathy. When manufacturers and engineers go to a chemist, and ask him to make an investigation, and screw him down to the lowest point, turning the equivalent of his brains into cents and mills, they usually get an exact equivalent in poor work for miserable pay, and no one has or should have any sympathy with them, and the manufacturer under these conditions has no right to ask for any, and no reason to criticise any work that he may get under such circumstances. But I am disposed to think that the chemists who have been represented and discussed at this meeting do their work conscientiously, and that it is as reliable as that of almost any profession.

I believe, however, that this problem of steel rails is being investigated in the wrong direction. I said so at the Pittsburgh meeting, and I think the discussion of this meeting will prove it to all those who have heard it. I think the chemist is incapable of solving this problem unless he goes very far into the domain of physical chemistry, so far that it becomes physics and not chemistry; and that the physicist will be the one on whom we must rely in the future for the elucidation of the subject. The chemist may aid the physicist, but it is my decided opinion that we are looking in the wrong place to get the explanation of the phenomena.

Attention was called at the St. Louis, and afterwards at the Baltimore meeting, to the fact that the pounding which a rail receives from the falling of the engine from a high rail to a low one was sufficient in many cases to account for effects which have not been explained in this discussion. This statement was made after an extended observation had been made of rails laid over many hundred miles of railway in Europe. But if it is true, as Mr. Cloud stated in his papers read yesterday, that the blow of the engine is repeated not only at the end of the rail, but every time the driving wheel makes a revolution, it

will explain much of the giving-way of the rails over their whole length, and the effects of these blows on the physical condition of the steel should be very carefully investigated. It was shown in the discussion of the law of fatigue and refreshment of metals at the meeting in Montreal, that every blow was accompanied by a physical effect which could be rendered distinctly visible. The blow and pressure of the gag which always leaves its trace on the rail is certainly less of a physical effect than the constant and rapidly repeated series of blows which the rail receives from the continual passing of trains, yet the gagging always injures a rail and sometimes destroys it.

At the Baltimore meeting two rails were shown which had been placed in essentially different conditions, and which had been subject to a cold flow of the metal in every part of the rail even to the very outside of the foot. Mr. Metcalf yesterday showed this same kind of a flow in rolls. He also spoke of the ignoring of the copper in the analyses of steel rails. At the Washington meeting in 1876, the statement was challenged that good steel rails had been made containing any large percentage of copper, and though repeatedly promised the analyses of such rails, they have never been produced. Some years ago, in visiting one of the largest steel works where the ingots are compressed, I noticed a jet of blue flame passing out from the bottom of the ingot mould, which I at first thought was phosphorus, but which I afterwards determined to be copper. Certainly, if there is enough copper in the rail to allow of its becoming visible in the color of the flame, there must be enough to influence its physical condition and its life, and we cannot afford to neglect it.

I have also had occasion to show that bubbles produced in the steel ingots were never rolled out of them, and that if they were once in the ingot they remain in the rail and arrange themselves in such conditions that they were sure sooner or later to engender a weakness in the direction which the cavities take. As these bubbles are never rolled out as the rail wears, they come to the surface, and the rail which shows a chemical analysis which is perfect, becomes imperfect from a physical defect. Every one who is familiar with the breaking of metals in a testing machine has been made aware of the fact that these bubbles will often cause metals whose analysis is faultless, to break with a very low tensile strength, while a piece without bubbles, taken from the same sample, will break with a high one. These bubbles are sometimes finer than pin-holes. I have, in investigating this fact in

railroad material, occasionally come across iron and steel which were so fatigued at the time that they left the rolls, that they were really unfit for service; and I have seen new rails which I should not feel justified in placing in any other position than on a side track. This question of fatigue in the course of manufacture has not been discussed at all so far as I know. I took occasion to show in the St. Louis meeting, that a rail taken from the Northern Railway of France, which I had the pleasure of examining, and which was condemned as good for nothing, and which the manufacturers were obliged to replace, when brought to Paris and submitted to chemical and physical tests, proved to be as good as any rails which were in the service. These kinds of physical defects are the ones which are to be looked after; and, in fact, nearly all the defects for which a chemical solution is demanded are physical. I have within the last year had occasion to examine metals and alloys which are fatigued, and find that while chemically the same, they act physically so entirely differently that I hope at some future meeting of the Institute to bring the matter before you.

I am at a loss to know why certain chemical substances which we know are in the iron or are likely to be there, should not be determined, and their effects discussed. Copper is one of these, sulphur is another, titanium and vanadium are known to be sometimes there. Why should not the effects of these substances be discussed?

Mr. Metcalf has alluded to the question of occluded gases. I have had occasion to show recently in my laboratory that as a general thing metals and alloys which were brittle from fatigue, contained a much larger amount of gas than the same metal which was not fatigued. I have also ascertained that the metals in this condition go into solution in a manner quite different from those not fatigued. I think also that the place from which the sample is taken from the rail will make some difference with regard to the results. Some one, I do not remember who, made an investigation some years ago upon the effect of having the rails always rolled in the same direction, and also of having been rolled backwards and forwards, and showed that under the latter course there were of necessity weak spots somewhere near the centre of the rail, yet in all this discussion the methods of rolling have been passed by almost in silence.

Mr. Sandberg in his paper mentions the idea of using a registering dynamometer attached to the punching machines, and of determining the quality of the metal by the resistance which is shown. I think the

first idea of this kind was published by Professor Langley, then of Pittsburgh, who, while making some investigations for Messrs. Miller, Metcalf and Parkin, announced as the result of a series of dynamometric experiments that abrasive resistance was the term which should be used in regard to steels of different wearing qualities. I have had extremely delicate dynamometers attached to the instruments of precision with which I am making the investigation on the fatigue of metals, and hope soon to communicate the results of the investigation made with them to the Institute.

I wish again to call attention to the fact that we are using the words "hardener" and "hardness" without any real idea of the meaning of these words. When we say hard and soft, as we have been constantly doing during this discussion, is it quite sure that every one has exactly the same meaning in his mind? Certainly, when hard is used in distinction from soft, we mean not the capacity of wear, but the capacity of resistance to penetration, to fracture, or some other resistance, and do not always mean the capacity of resistance to abrasion or crushing, which the discussion would sometimes seem to imply were the only qualities requisite to constitute a good rail.

J. W. Cloud, Altoona, Pa.: I would call the attention of the Institute to the title of Dr. Dudley's interesting and valuable paper,—"The Wearing Capacity of Steel Rails in relation to their Chemical Composition and Physical Properties." Here are two separate and distinct questions: 1st, The wearing capacity of steel rails in relation to their chemical content; and, 2d, the wearing capacity of steel rails in relation to their physical properties.

The discussion has been almost exclusively confined to the former question, on which there may be many differences of opinion in matters of detail without greatly affecting the result. The latter question has been ignored, and I wish to call attention to it, particularly, because it is the practical question after all from the consumer's standpoint, and because the paper under discussion is more decisive on this question than on the other. In fact, I think we must admit that Dr. Dudley has established his main point, viz , that the softer steel, physically, gives the slower wear, contrary to general belief among engineers in this country.

Of course, we are all interested in the whole subject from a scientific standpoint, but as the discussion has been participated in largely by

manufacturers of steel, with a business animus, and as they have endeavored to overthrow Dr. Dudley's chemical recommendations because they are necessarily the most vulnerable points in the paper, I wish to recall attention to the facts of the physical properties as being the consumer's only practical guide, and as affording the most conclusive results in the data before us.

The consumer of steel rails cannot test every blow chemically, but he can do so physically, and it is my opinion that a bending test of a whole rail section, say three feet long, under steady pressure, instead of a drop, with a specified deflection, without cracking, and a load not to be exceeded to produce this deflection for each rail section, will be a good practical test, not burdensome to manufacturers, and one that will insure consumers such a degree of softness in steel as they may desire.

They should not attempt to dictate to manufacturers how this degree of softness shall be obtained chemically, but allow them full freedom to do as they please, so the proper physical properties are had. Further, we are now in possession of the information requisite to make such specifications with the certainty of greater safety as well as more economical results in the life of rails.

I have recently had opportunities at Altoona to see other and very convincing evidences of wear, as compared with physical softness. The locomotive driving-wheels probably cause at least one-half of the wear of rails, and the forces which go to wear the rails from the tires, must react with similar intensities on the tires themselves. We therefore have in driving-wheel tires an opportunity to see the wear in a more concentrated form, so to speak, or with rates as well as differences magnified. I have recently found differences of one inch to two inches in circumference of two tires on the same axle when coming to shop for turning, and it is invariably evident that the smaller tire is much the harder, the chips from it being short and brittle, while chips from the larger tire are much longer and tougher. In the worst case I have observed, viz., two inches difference in circumference, this difference in hardness, as observed from the cutting, was more marked than in the other cases. Tires are always grouped in sets by the manufacturers from their knowledge of the chemical composition of the steel, with an attempt to get those in one set which have the same degree of hardness, so that the wear shall be equal all around; they succeed pretty well on an average, but I have been noting the exceptions.

JACOB REESE, Pittsburgh, Pa.: I have been very much interested in the reading and discussion of Dr. Dudley's paper. As far as it relates to the data of work performed by the rails, and the determination of their physical and chemical properties, I have nothing but commendation of Dr. Dudley to express, as the investigation covered a greater range, and was performed with more care in detail, than any similar work which has come under my notice. But I beg leave to differ with Dr. Dudley in his conclusions.

What are the factors of hardness? Are they not carbon, silicon, phosphorus, and manganese? Now it is an undisputed fact among metallurgical experts that pure carbon and pure iron make the best steel of all degrees of carburization, and for all purposes. While carbon hardens, it also strengthens the metal, but silicon, phosphorus, and manganese, in hardening, make the metal also brittle, and are injurious in any amount. Carbon should be called a *strengthener;* and I claim that a steel-rail made hard with carbon, with the other three hardeners absent or reduced to a minimum, will carry a greater tonnage than any of Dr. Dudley's soft rails.

But until the basic process is put into operation in this country we cannot expect to produce Bessemer or open-hearth steel without the presence of silicon, phosphorus and manganese, in considerable quantities, and I greatly doubt the possibility of reducing the percentage of any of them by the present practice without seriously diminishing the output, and correspondingly increasing the net cost of production; which is an important question, since the increased life of the rail may be more than balanced by its increased cost.

I think that the soft rails performed a greater amount of work, because they contained a less amount of silicon, phosphorus and manganese (*brittlers*, if I may so term them), and that carbon does not reduce the wearing capacity of rails. I believe that a rail made by the basic process, with silicon, phosphorus and manganese reduced to a minimum, and containing 0·60 carbon, will be stronger and tougher, and will carry double the tonnage of any of Dr. Dudley's soft rails.

(To be continued.)

Pronunciation of Foreign Languages.—The Polyglot institute at Paris proposes to try a phonograph of new construction, in order to teach its pupils how to pronounce correctly the difficult words of foreign languages.—*Chron. Indust.* C.

BOILER EXPLOSION AT GAFFNEY & CO.'S DYE WORKS, PHILADELPHIA, JUNE 1, 1881.

By W. Barnet Le Van.

A cast iron flat boiler head only two inches thick would not be considered by an experienced engineer strong enough for a cylinder boiler thirty-six inches in diameter, especially when the rim by which it was secured to the shell plate was only 1·625 (1⅝) inches thick, with scarcely any fillet at its junction, and this rim being placed so as to project into the shell of the boiler, when it should have projected outside, according to the usual practice.

The insufficiency of such a boiler head was proved, with fatal results, on the 1st of this month, at the dye works of the above-named firm. The head of this boiler blew out, killing three persons and wounding eight, besides damaging property to the amount of about $15,000.

The boiler, which was new, was built by Messrs. Sidebottom & Powell, of Frankford, Philadelphia, for Gaffney & Co., and was inspected by a Steam Boiler Insurance and Inspection Company on the 2d day of March, 1881, and tested in connection with two similar ones on the 7th of the same month, by hydraulic pressure of ninety-five pounds per square inch, and was passed by this Insurance and Inspection Company as capable of carrying a working steam pressure of sixty-five pounds per square inch above the atmosphere.

During the examination of this company's inspector, before the Coroner's Jury, he stated, in answer to a question from one of the jurymen, that "there were quite a number of boilers in this city which had cast iron heads, and that his experience had taught him that a concave cylinder boiler head was stronger than a flat head." Yet he passed this flat cast iron head without any comment at the time to the attendant owners or the insurance company in whose charge it was placed.

This disc of cast iron (the boiler head) took its full load of about 30 tons without any other support than that at the connection of the rim with the shell of the boiler.

The cylinder head of a sixteen inch diameter engine would have

been made one inch thick, and yet this boiler head, having twenty times the area, upon which the pressure was exerted through a three-fold greater leverage, was but twice as thick. A thirty-six inch engine cylinder head would have been cast hollow concave, or else stoutly ribbed on its face; yet this boiler head was a plain flat disc, only two inches thick, and the rim, which is at right angles to the head, only 1·625 ($1\frac{5}{8}$) inches thick, with scarcely any fillet at its junction, and this rim being placed inside of the boiler the steam pressure acted on it by tension, in place of by compression, at its weakest point. Had the pressure acted by compression on the rim of the head, it would have resisted nearly twenty per cent. more pressure before parting. An examination of this head after the explosion shows that it was under internal strains resulting from bad design as to form and proportion.

It is a well-known fact in mechanics that, in casting plates of iron, if one part is less in thickness than the larger portion of the casting, especially when two surfaces join each other at right angles, as was the case with this boiler head, the difference in cooling will cause internal strains, in some cases to such an extent that when a casting becomes cool it may break by the unequal contraction of the several parts, without any pressure whatever. The effects of these strains to reduce the ultimate strength of the boiler should be taken into account as an important element, as in this case a part of the factor of safety was already expended, by reason of the improper construction of the boiler head in this respect.

Another great defect in this boiler head was that the man-hole plate was not properly fitted to its seat on the head; the seat was a planed surface, and the man-hole plate was a wrought casting, so that in making a steam-tight joint between the two a strain was induced additional to that due to the improper design and proportions of the head, as before explained. Had this boiler head been made concave, and of an average thickness of 1·25 ($1\frac{1}{4}$) inches, with the convex side inward, with a round man-hole in place of an oval one, and thickened around the same on both sides, I have no doubt whatever it would never have given away; but, better still, it should have been made of wrought iron, dished, and with the convex side projecting from the boiler.

Now that a board of engineers is about to revise the boiler inspection laws of this city, would it not be well to add a clause providing that cast iron should be dispensed with altogether in the construction

of steam boilers, unless so employed as to conform with well-known and approved design and proportions, and so placed in the boiler as to resist the greatest amount of steam pressure with the least amount of metal?

The writer does not hold cast iron to be an improper material, but from the fact that its proper form for resisting heavy pressures is not understood by the majority of our boiler makers, and even with the most skillful moulders a perfectly homogeneous casting is the exception and not the rule. The thinner a casting is made the more homogeneous it will be, all things being equal. Assuming the form and design to be unexceptionable, the use of cast iron is attended with greater risk than that of wrought iron, and should be regulated by such judicious proceedings as will tend, as far as possible, to render its employment (which is often convenient and desirable) sufficiently safe to be unobjectionable upon the grounds above referred to.

Philadelphia, June 8th, 1881.

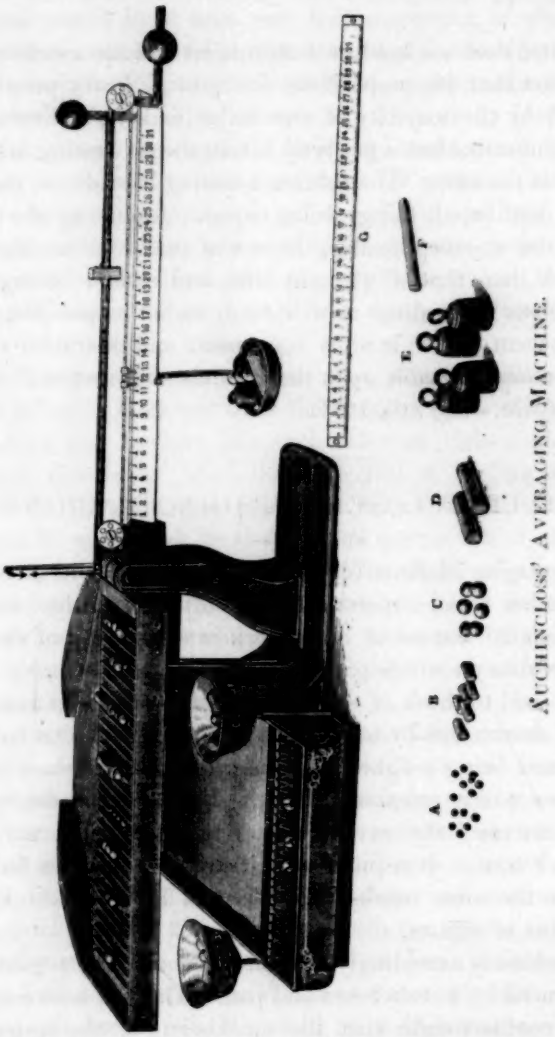
AUCHINCLOSS' AVERAGING MACHINE.

The Averaging Machine (or Book-keeper's Assistant) gives mechanical expression to an important branch of mathematical science, and employs manual instead of brain work in the process of determining the average date on which payment falls due.

It seems odd to think of so imponderable an element as an *average date* being determined by material objects, such as leaden balls, or bits of metal, and being weighed out like articles of merchandise. This, however is not only practicable, but the apparatus will determine with unfailing accuracy the average dates of more than one hundred accounts per hour. It requires no skilled mathematician for its manipulation, as the same results can be secured by those who know little about the use of figures.

The machine is exceedingly simple. It consists of a grooved platform, balanced by a scale beam and pan. The pan is so coupled with a moving counterweight that the equilibrium of the system is preserved for all positions of the pan. The weights bear to each other the ratios of 1 to 10 and of 1 to 100, so that they may be used to represent units, tens, hundreds; tens, hundreds, thousands; hundreds, thousands, tens of thousands, and so on.

The grooves of the platform represent the days of the month, and the notches on the scale beam the average dates. In solving any



AUCHINCLOSS' AVERAGING MACHINE.

problem, the weights are first distributed over the platform so as to represent the several purchases made on the respective days. The scale pan is then laden with an amount *exactly* equal in weight to the entire load upon the platform. After the machine has been thus

laden the scale pan should be moved along the scale beam until it reaches a point where the weights are in equilibrio. The reading of the scale beam gives the average date of the purchases.

The entire process requires so little thought that one can determine a large number of averages without fatigue, and can verify the work by simply glancing a second time over the platform and pan for re-assurance as to the proper location of the weights. After the solution of any problem the weights are dumped and screened, thus preparing the machine for the next solution.

It is curious to note that the same machine can be used for solving a great variety in simple and inverse proportion, besides many others of a more intricate character, all of which may be determined with the greatest rapidity.

RADIO-DYNAMICS. II.

By Pliny Earle Chase, LL.D.

Abstract of Lectures delivered before the Franklin Institute, March 10 and 17, 1881.

Continued from page 65.

We have confined ourselves thus far to the simple dynamic laws of radial action to or from centres of energy and of inertia. I hope to satisfy you that those laws are sufficient for the explanation of all varieties of physical phenomena, and that radio-dynamics is, therefore, the foundation of all dynamics, the UNIVERSAL physical science, of which photo-dynamics, thermo-dynamics, electro-dynamics, cosmo-dynamics, moleculo-dynamics and chemistry are branches.

Following the Baconian method, I will first lay before you some general principles, followed by some additional FACTS to which I have been led by applying mathematical principles to the study of radio-dynamics, and then interpret those facts, in the hope of making them helpful towards the strengthening of hypotheses and the discovering of new facts or laws.

The elements of physical energy are mass, m, and velocity, v. The total energy of any force is called its *vis viva*, or living force. It is measured by the *work* that it is able to accomplish against uniform resistance, and is represented by $\frac{mv^2}{2}$.

La Place defined velocity as "the ratio of the space to the time

employed in describing it." In recent dynamical treatises the space traversed, or length, is represented by l; time of traverse, by t; velocity, by $\frac{l}{t}$; energy, by $\frac{m\,l^2}{t^2}$.

The greatest or controlling energy must, of course, be represented by the product of the greatest mass by the square of the greatest velocity.

The greatest mass of which we have any practical knowledge is the mass of the Sun, M_0; the greatest centripetal acceleration is the force of gravity at Sun's surface, G_0; the greatest velocity of wave propagation is the velocity of light, or the velocity of radiation, V_0, at the seat of greatest centripetal acceleration.

Action and reaction being equal and in opposite directions, we may reasonably look for some simple relation between the centripetal and centrifugal maxima, G_0 and V_0.

If we regard Sun's radiating energy as an action, its reaction must be dependent upon its inertia, or mass.

The velocity of projection against uniform gravitating resistance is represented by gt, t being one-half the time of flight. The height of projection is $\frac{gt^2}{2}$. The height of projection which would give the velocity of wave propagation in an elastic medium is one-half the "height of a homogeneous atmosphere" of the medium.

Every particle of the Sun's surface is continually solicited by gravitating tendencies, G_0 and G_u, towards its own centre and towards the centre of the universe. In each solar rotation the particles are alternately projected from and drawn towards the centre of the universe, C_u.

We may, therefore, let T_0, $= \frac{1}{2}$ solar rotation, represent the time of cyclical equality of action and reaction between solar inertia and universal inertia, or between solar gravitation and æthereal undulation.

Then $L_0 = G_0\,T_0^2$ is the height of a homogeneous æthereal atmosphere, at Sun's surface, which would have a velocity of wave propagation equivalent to the velocity of light; $\frac{L_0}{T_0} = V_0$ is the velocity of light.

All forms of energy, mechanical, thermal, photic, electric, magnetic

or chemical, are derived from and can be compared with the maximum energy, $\frac{M_0 L_0^2}{T^2}$.

1. All astronomical, barometric or other mechanical estimates of solar mass and distance involve the proportionality, $g \propto \frac{m}{l^2}$.

2. The arbitrary units of thermo-dynamics are based upon the work done against the centripetal accelerations of superficial terrestrial gravity. The unit of acceleration, or the sum of accelerations in unit of time, at unit of distance, is proportioned to mass. Therefore, if we designate Earth's mass by m_3, we have the proportion

$$M_0 : m_3 :: V_0 : u_3$$

V_0 is the velocity acquired during the cyclical actions and reactions of solar condensation and æthereal elasticity, at Sun's surface; $u_3 =$ ·56558 mile is the velocity acquired during the cyclical actions and reactions of water congelation and vaporization at Earth's surface. Dividing by $\sqrt{180}$ for the Fahrenheit scale, or by $\sqrt{100}$ for the Centigrade scale, we have the arbitrary units of velocity, ·042156 mile for 1°F., ·056558 mile for 1°C. The equation $V = \sqrt{2gh}$ gives $h =$ 772 ft. for 1°F., or 1389·6 ft., = 424 metres, for 1°C.

Combining these heights with the arbitrary units of mass, we have J, = 772 ft. lbs., for the English thermal unit, and C, = 424 kilogrammetres, for the calorie or French thermal unit.

3. Thermal, mechanical and photo-dynamic energies may be compared with energies of chemical combination, through the ratios

$$M_0 : m_3 :: V_0 : u_3 :: h_0 : h_c$$

Earth's mean distance from Sun, or height of solar projection, is represented by h_0; h_c is $\frac{1}{4}$ of $\frac{5}{9}$ of the height to which water vapor would be thrown, against the retardation of gravity, by the combining energy of oxygen and hydrogen, the two constituents of water; $\frac{1}{4}$ is the length of a conical pendulum which would vibrate in the same time as a linear pendulum of unit length; $\frac{5}{9}$ is the ratio of *vis viva* of wave propagation to the mean *vis viva* of the oscillating particles which originate the waves.

4. Cosmical, electrical and photo-dynamic energies may be compared by means of the ratio

$$M_0 V_0 v_3 : m_3 V_0^2 :: m_3 V_0^2 : m_3 V_0 v_3$$

Sun, M_0, is at the centre of nucleation in the solar system; Earth,

m_3, at the centre of condensation; Jupiter, m_5, at the nebular centre; v_3 is Earth's aphelion or "nascent" orbital velocity.

5. The electro-static, magnetic and electro-kinetic units of energy can all be derived from the above expression for Earth's photo-dynamic energy, $m_3 V_0^2 = \frac{m_3 L_0^2}{T_0^2}$, through the equations $[e E] = [m \Omega] = [p C] = \frac{M L_0^2}{T_0^2}$.

The bracketed symbols represent, respectively, quantity of electricity; line integral of electromotive force, or electric potential; quantity of free magnetism, or strength of a pole; magnetic potential; electro-kinetic momentum of a circuit; electric current.

6. Atomic energy, or energy of unit volume, can be compared with Earth's photo-dynamic energy of unit volume, $\frac{m_3 L_0^2}{r_3^3 T_0^2} = \frac{m_3}{L_1 T_0^2}$, and with corresponding electric energies, through the equations

$$[D E] = [B H] = [C V] = \frac{M}{L T^2}$$

The bracketed symbols represent, respectively, electric displacement (measured by surface density); electromotive force at a point; magnetic induction; magnetic force; current electric intensity at a point; vector potential of electric current.

7. Electro-chemical and electro-magnetic energies may be compared with thermal, photo-dynamic and other energies through the proportion

$$\varkappa : \mu :: M_0 t_a^2 \times M_0 v_3^2 : m_3 t_n^2 \times m_3 V_0^2$$

I designate Weber's units of electro-chemical and electro-magnetic force by $\varkappa$ and μ, respectively; t_a is the time of acquiring orbital velocity, or incipient associative energy, at Laplace's limit of equal velocities of rotation and revolution; t_n is the time of acquiring nucleal nascent or dissociative velocity. The ratio of t_a to t_n is the same as the ratio of the diameter of a circle to its circumference, $1 : \pi$.

8. Total magnetic force, φ_0, can be compared with the reactions of terrestrial magnetic force, φ_3, by the proportion

$$M_0^2 : \pi^4 m_3^2 :: \varphi_0 : \varphi_3$$

The reactions of orbital tendency are $t_a M_0^{1/2}$, $t_n m_3^{1/2}$, respectively. Centripetal undulation varying as the fourth power of orbital velocity, we have the ratio $t_a^4 M_0^2 : t_n^4 m_3^2$, or $M_0^2 : \pi^4 m_3^2$.

9. In the actions and reactions between the centre of nucleation, M_0,

and the centre of condensation, m_3, there are continual tendencies towards the centre of gravity, the centre of lineal oscillation and the centre of conical oscillation, as well as to centripetal and orbital motions. These are all satisfied by the proportion

$$M_0 : m_3 :: (2\times3\times4)^4 : 1^4 :: 331776 : 1$$

In order to show the closeness of accordance between the theoretical results and those which have been deduced from observation or experiment, I make the following comparisons:

1. The theoretical oscillatory value of Sun's mass, (9), gives 92,785,700 miles for Sun's distance; 8·809″ for Sun's parallax; 25·496 days for Sun's rotation; 299,943 kilometres per second for the velocity of light. These values differ by less than $\frac{1}{40}$ of one per cent. from Faye's, Michelson's and La Place's estimates.

2. The difference between the theoretical and experimental thermodynamic velocities is less than $\frac{3}{4}$ of one per cent. Perhaps, when proper allowances are made for the temperature at which the experiments were performed, the accordance will be found to be exact.

3. The difference in the combining energy of hydrogen and oxygen is less than $\frac{1}{85}$ of one per cent.

4. The difference in Earth's "nascent" velocity is less than $\frac{1}{11}$ of one per cent.

5, 6. The difference which is indicated by the latest electro-dynamic and electro-magnetic investigations is less than $\frac{1}{50}$ of one per cent.

7. Weber's experimental determination of the electro-chemical unit was $\frac{2}{5}$ of one per cent. greater than the theoretical value.

8. The experiments of Joule and others, in Great Britain, upon magnetic action and terrestrial reaction, indicate a difference of about $1\frac{1}{4}$ per cent. in the estimate of Sun's mass, and $\frac{5}{12}$ of one per cent. in the estimate of Sun's distance.

I have been obliged to use many technical terms and equations which are doubtless new to most of you, but you can all see how close is the agreement of the values which are derived mathematically from simple radio-dynamic action, with those which have been obtained experimentally by the most careful observers. You will have noticed how often I have called your attention to evidences of the fourth fundamental law, which is generally known as "Fourier's Theorem," viz.; every periodic vibrating or orbital motion can be regarded as the sum of a certain number of pendulum vibrations.

To illustrate this law I have prepared a stretched cord, to which I

have suspended three balls, at equal intervals, by strings of equal length. I set the middle one swinging, and you see how it gradually imparts its motion to the two lateral balls, and comes to rest itself. Now the side balls are losing their motion, and the middle one takes it up again, until it swings almost as far as at first, and its companions hang motionless. These alternations continue until the resistance of the air overcomes the motion, and the balls are all quiet, as at the beginning. The motion, however, has not ceased; it has merely been imparted to the air, and by the air to the æther, so that we do not see it, and we are unable to follow it through all its changes.

When the regularly recurring vibrations of an elastic medium are transmitted to the ear through a suitable medium, they produce the sensation of sound; if the interval between the successive vibrations is short enough, the sound is musical. We may distinguish between the music in the sounding body and the music which we hear. A bell may ring when swung by the wind, but it will give no sensation of sound unless its vibrations reach the drum of the ear with energy enough to make it vibrate. I strike this tuning fork, but you hear nothing except a single ring. I bring it near the opening of this wooden box and the oscillations of the fork are imparted, first to the air, then to the wood, and the note of the fork is sounded, loud, clear and musical.

It is not necessary that the unison should be exact, or, in other words, that the fork and the resonator should make the same number of pendulum swings in the same time. If the vibrations coincide at intervals which are so short that the ear cannot distinguish between them, one body, for example, making three vibrations and another two in the hundredth part of a second, both sounds are said to be harmonious. Whenever any elastic medium is vibrating, its waves tend to produce harmonic vibrations in all bodies which they strike.

If the elastic force of the æther is more than a million million times as great as that of the air, its musical rhythm, if our ears were sensitive enough to hear it, should be of a far higher order than that of any earthly choir. Chemical atoms and molecules, the particles of heated or electrified bodies, satellites, planets, suns, stars and nebulæ, all intercept the luminous waves. Hence there should be a continual tendency to relative positions which would make all the motions harmonic, or rhythmical. Such a tendency is shown in atomic weights, spectral lines, mechanical equivalents of heat, the various phenomena

of electro-dynamics, the relative positions of satellites, asteroids and planets, and the bonds of union between our own system and the nearest of the fixed stars. The truth of the radio-dynamic hypothesis has been further confirmed by the indication or prediction of harmonic nodes beyond Neptune, as well as between Mercury and the Sun, fourteen of which have been subsequently corroborated by the calculations of various European and American astronomers.

If you throw a ball, or fire a projectile upwards into the air, it will continue to rise until the resistance of the air and the attraction of gravitation have overcome the velocity of projection. If there were no air, and no resistance to the upward flight except gravitation, the relation of projectile velocity to gravitation and height of projection would be represented by the equation

$$v = \sqrt{2gh} \qquad (1)$$

The relation of velocity to time of rise, or to the equal time of fall would be represented by

$$v = gt \qquad (2)$$

In solar rotation each particle is alternately projected from and drawn towards the universal centre of gravity. Substituting the equatorial values of g and t in equation (2), we find that v is *the velocity of light.*

The circular-orbital velocity of a particle at Sun's surface, v_o, is connected with the velocity of rotation, v_r, and the velocity of light, v_λ, by the equation

$$\pi\, v_o^2 = v_r\, v_\lambda \qquad (3)$$

The velocity of a particle, at any distance whatever from any attracting centre, can be deduced from equation (3), by means of the eleventh fundamental law, that centripetal accelerations vary directly as the mass and inversely as the square of the distance. Hence we see that *all gravitating velocities are dependent on the velocity of light.*

The distribution and motions of the principal planetary masses are also dependent upon the same velocity. The largest planet is Jupiter, which is more than twice as large as all the other planets. The mean centre of gravity of the solar system is therefore the same as the mean centre of gravity of Sun and Jupiter. The next planet in point of magnitude is Saturn, which is more than two and three-fourths times as large as all the remaining planets, and which is placed at the nebular centre of planetary inertia. When the primitive planetary belt was successively divided into inter-asteroidal and extra-asteroidal belts,

two-planet belts and single-planet belts, the amount of inertia remained unchanged. But in these divisions the rectilinear propagation of luminous waves has been changed into the synchronous oscillations of conical pendulums, introducing the length-ratio of 4 to 1; the synchronous oscillations have been changed into orbital oscillations, in which the time varies as the $\frac{3}{2}$ power of the distance; nebular radii have given place to radii of subsidence-collision, which are only $\frac{2}{3}$ as great; and centripetal tendencies vary as the fourth power of orbital tendencies. If we designate Saturn's mass by m_6, and the mean velocity of rotation of the centre of gravity of the solar system by v_s, these changes and their dependence on the velocity of light are shown by the proportion

$$4^{\frac{3}{2}} M_0 : (\tfrac{2}{3})^4 m_6 :: v_A : v_s$$

Bessel's estimate of the comparative masses of Sun and Saturn was $M_0 = 3501{\cdot}6\ m_6$. Substituting this value in the above proportion, we find

$$v_A = 141815\ v_s$$

The most accurate estimate that we are able to make of v_s is 1·31405 miles per second. This velocity, which is so readily deduced from the velocity of light, is also *the wave-velocity which represents the conversion of water into vapor.*

Let θ represent the latent heat of steam, then well-known laws of thermo-dynamics give us the equations

$$v_s = \sqrt{2gh}$$

$$\theta = \frac{h}{1389{\cdot}6}$$

Solving these equations, we find

$$h = 750{,}098 \text{ feet.}$$

$$\theta = 539{\cdot}794 \text{ C.}$$

Among the various experimental values which have been obtained for θ, the following are generally regarded as the most trustworthy:

Favre and Silbermann,	535·77°
Andrews,	535·90°
Regnault,	536·67°
Tyndall,	537·20°
Despretz,	540·00°
Dulong,	543·00°

According to the kinetic theory of gases the internal movements of the particles of steam are rectilinear. When the steam is condensed, in the form of water or ice, the internal energies tend to maintain a spherical figure. The synchronous oscillations of the two conditions may be represented, according to Fourier's theorem, by linear and conical pendulums. The height to which the vapor would be projected above the water level being represented by 750,098 feet, or 142·064 miles, the length of the conical pendulum is $\frac{1}{3}$ as great, or 47·355 miles, and the length of the linear pendulum is $\frac{4}{3}$ as great, or 189·419 miles. The heat of sphericity should be, therefore, $\frac{1}{3}$ of the latent heat of steam, or 179·93°. Deducting 100° for the expansion of water from the freezing to the boiling point, we have 79·93° for the "latent heat" of ice, or more properly speaking, for the heat which is required to overcome the crystallizing energies of water. The following values have been obtained experimentally:

Desains and De la Provostaye, . .	79·25°
Black, ...	79·44°
Person, ...	80·00°
Hess, ...	80·34°

The combined influence of photo-dynamic *vis viva* and the nebular "subsidence" which was pointed out by Herschel, is shown in the proportion

$$1{\cdot}01555^2 : (\tfrac{5}{9})^2 :: m_5 : m_6$$

The radius of incipient subsidence for Neptune's orbit (Law 10, p. 61) is 1·01555; $\frac{5}{9}$ is the ratio of the *vis viva* of wave propagation to the mean *vis viva* of oscillating particles (Law 22, p. 62). If we substitute Bessel's estimate for m_6 in the above proportion we find

$$1{\cdot}03136 : \frac{25}{81} :: \frac{1}{1047{\cdot}9} : \frac{1}{3501{\cdot}6}$$

If we take Stockwell's estimate of Neptune's radius of incipient subsidence, 1·0145, we get $M_0 = 1050\ m_5$. Bessel's estimate was 1047·879; Leverrier's, 1050. The difference between the two estimates is only $\frac{1}{5}$ of one per cent. Bessel's is the one which is adopted by the British and American Nautical Almanacs, but the reputation of Stockwell and Leverrier seems to render it probable that the true values may be intermediate between the two which are here given. The photo-dynamic relations of mass and distance become still more striking when we find that the incipient subsidence at the nebular centre, Jupiter, is at a mean proportionate distance between the centre

of condensation, Earth, and the incipient subsidence of the primitive planetary belt, Neptune. This is shown by the proportion

$$1 : 5{\cdot}52 :: 5{\cdot}52 : 30{\cdot}47$$

If we represent Earth's semi-axis major by 1, Jupiter's secular aphelion is 5·52, and Neptune's secular aphelion is 30·47.

These comparisons might be extended almost without end. We have now surveyed the whole field of physical science, and have found, in every direction, that all possible physical energies can be expressed in terms of the greatest and most pervasive energy, through the mass of the Sun and the velocity of light. Simple gravitation, solar rotation, nebular subsidence, orbital revolution and all other gravitating motions, cosmical aggregation, the distribution of planetary masses, the establishment of centres of inertia, condensation and nucleation, evaporation, crystallization, heat, mechanical work, barometric pressure, atomic energy, chemical combination, electricity and magnetism, are all so simply connected by the universal laws of action and reaction in elastic media, that they all furnish ready methods for estimating the mass and distance of the Sun and the velocity of light.

THE PROPERTIES OF AIR RELATING TO VENTILATION AND HEATING.

By ROBERT BRIGGS, C.E.

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The surface of the earth is covered by a gaseous body, some forty or fifty miles in depth, which is called the atmosphere. Chemistry has discovered and isolated various gases, some of which, so far as further separation is concerned, may be deemed elementary, while some are chemical compounds of definite proportions of other elementary gases and bodies. In some cases bodies which in their elementary form, at temperatures subsisting in nature, are solid, become portions of chemical combinations as gases at similar temperatures.

The atmosphere is composed mainly of a mixture of two elementary gases, together with small but appreciable quantities of two other gaseous bodies, products of combustion; beside other gaseous bodies of various kinds, in nearly inappreciable quantities, the latter varying somewhat in character in inhabited localities. Its substance, as a

whole, is a compound gas of nearly uniform composition known as the AIR.

The air, when uncontaminated by local causes, has been found by the most painstaking and careful observations, in all parts of the earth, and at all heights, from the level of the sea to the top of the highest mountains reached by man, or the greatest elevation attained by the balloon, to have identically the same components. Omitting the two smaller constituents, in 100 volumes of air there are 79·1 of nitrogen and 20·9 of oxygen, or in 100 parts by weight there are 76·9 of nitrogen and 23·1 of oxygen; oxygen being heavier than nitrogen in the proportion of 16 to 14. These proportions differ a little from a chemical compound of four parts (weights) of nitrogen to one part of oxygen, and beside this difference it must be stated there is certainly no chemical combination of the gases in air—they are simply intermixed. All gases or gaseous bodies mix with each other indefinitely and perfectly, whatever may be their relative densities or weight, a difference in the most extreme case of over 250 to 1, and they never separate from each other, wholly or partially, except by condensation of some of them from a gaseous form (or vapor) to that of a liquid by reduction of temperature or increase of pressure or both.

Beside nitrogen and oxygen in the air, there is always present carbonic acid and vapor of water. Of the carbonic acid the quantity is quite variable, but very small in all cases. Pure country air has an average of from $3\frac{1}{2}$ to 5 parts by volume in 10,000—4 parts being considered by most physicists as a proper quantity to adopt as appertaining to *pure* air. Four per cent. of one per cent. may convey the idea to readers. While the quantity of vapor of water present is yet more variable, as it depends on the temperature of the air at the time as well as upon the locality, not only where the air may be taken, at any place of observation, but where the air came from, by the winds, to reach that place. The quantity of vapor of water present in air is called its humidity, and air is said to be saturated with humidity when it holds as much vapor as it can without its condensing into water as a liquid.

We commonly know vapor of water as *steam*. At 212° and under the ordinary pressure of the atmosphere (which we will speak more about hereafter) water boils, and if the temperature is maintained, it will all boil away in the air. But vapor of water exists in contact with water without boiling, at all temperatures, and if the pressure of

the atmosphere which rests upon it is taken away, water will boil at any temperature whatever, dependent on the extent of relief of pressure. It is a curious truth that water only exists as a liquid because it is held down by the pressure of the atmosphere upon its surface.

The natural temperatures of the climate we live in, omitting extremes, are, say, from 10° to 85° Fahrenheit. The quantity of vapor of water in each cubic foot of saturated air (at 30 inches of barometric pressure) has been ascertained with great care by eminent French physicists. This quantity is very small in any case, being only 4 per cent. by volume at 85°, and it falls off rapidly as the temperature falls; at 65°, or 20° fall of temperature, only half as much, or 2 per cent., can exist; at 45°, another fall of 20°, but 1 per cent. is found; at 28°, but $\frac{1}{2}$ per cent., while at 10° only $\frac{1}{4}$ of a per cent. of the volume of air can be invisible aqueous vapor. So much as even these small quantities do not exist in air generally, as the air which has derived its moisture from water or damp surfaces will, from the action of currents and winds, ascend to the upper, colder atmosphere, where it will deposit the same moisture by condensation, into clouds, with rain, hail or snow, when great quantities of moisture are condensed, and much loss of heat accompanies the position of the cloud as regards its elevation from the surface of the earth. The course of the winds will bring this dried air again near the surface at another place; so that the humidity of air in our country may at any time be only 30 or 40 per cent., or even less, of what would constitute saturation for air of the same temperature. The average humidity of the Eastern States is from 60 to 70 per cent. of complete saturation. The degree of saturation is measured by the *dew-point*, which is the temperature that is indicated by a thermometer, artificially cooled until a deposit of dew or condensed water appears on the bulb.

Air, as it is found in the neighborhood of our cities, and in the seasons of growth in the country, generally has very small quantities of other gases in its composition. The most general are ammonia, sulphuretted hydrogen and sulphurous acid gas, with numerous others of local derivation, especially near factories; but the quantities of such impurities present in the open air are even smaller than those given for carbonic acid or vapor of water, so that fresh air everywhere can be held, as before stated, to be mainly nitrogen and oxygen. Only the most delicate tests, where the hundredth of a per cent. is a unit, serve to measure the quantities of gaseous bodies vitiating air.

Beside the gaseous impurities referred to, there exists always in air of inhabited regions very small quantities of floating organic matter, composed of fragments of organic origin, vapors of the same source, like odors, for instance, microscopic germs or living organisms, together with dust of minerals or metals, smoke, etc., forming an insignificant part of the atmosphere, nearly inappreciable in amount by weight or measure, but of the greatest importance in effect upon the air of ventilation, as will be made to appear further on in this paper.

The main chemical characteristics of the gases in air are as follows: Oxygen is the most abundant element in nature. It forms, as stated, one-fifth of the atmosphere; it also is eight-ninths of the substance of water, and about one-half of all solid bodies of the earth—at least, of the crust of it so deep as we can investigate its formation. In its free state, and its existence in the air mixed with nitrogen can be considered free, it combines chemically with nearly all other elementary bodies. This combination is attended by evolution of heat, and is known as combustion.

Nitrogen, which is the chief constituent of the air, has few inorganic chemical combinations with other elements. It is an essential and considerable part of all animal tissues which are composed mainly of carbon, hydrogen, oxygen and nitrogen, and also an essential but very small part of vegetable tissues which consist principally of the first three bodies in the list.

Carbonic acid is the product of combustion of carbon, where two and two-thirds parts of oxygen by weight enter into combination with one part of carbon, also by weight, and form a colorless gas, about one and a half times heavier than air in equal volumes. It results from the burning of fuel—carbonaceous materials, either recent vegetable growths or the fossils of former vegetable growths—and from the slow oxidation of organic tissues called decay, beside being the chief product of respiration. Volcanic action, as well as some processes of combustion which take place in various localities under the surface of the earth, evolves large quantities of carbonic acid. On the other hand, while these sources of carbonic acid are in constant action, nature is restoring the equilibrium of condition; as all vegetable growths are absorbing carbonic acid, assimilating into wood tissues the carbon, and setting free the oxygen. It cannot be said, however, that the condition of the air is dependent upon vegetable growth to keep

down the proportion of carbonic acid, as it has been estimated that if the vegetable growth of the earth were to cease for two thousand years the effect of respiration and combustion in vitiating the air could only be detected by the nicest chemical analysis.

Carbonic acid is an innocuous gas, quite harmless to animal life except when it is substituted for oxygen in the air for breathing, and except in so far as its presence in large proportions interferes with the natural secretion from the lungs and possibly from the skin.

Vapor of water is the product of the combustion of hydrogen where eight parts of oxygen by weight combine with one part of hydrogen (the volume of the one part of hydrogen being twice that of the eight parts of oxygen). It is a colorless vapor or steam, about five-eighths as heavy as air, in equal volumes. When condensed as liquid water it is the chief constituent of organized bodies, forming the greater part of their weight. Water also plays an important part in the mineral kingdom as the water of crystallization of many minerals. Many substances dissolve in water. All animate creatures who live upon the surface of the earth require water as a liquid to drink, but the presence of vapor of water in the air does not seem to be absolutely essential to the existence of animals—except, perhaps, it may afford a mitigation of the extreme heat of the sun's rays as they shine through our atmosphere. But, on the other hand, all vegetable life demands, as a primary necessity, considerable vapor in the air, and in a warm saturated atmosphere it grows and thrives with the greatest luxuriance. Like carbonic acid, aqueous vapor is harmless to animal life, except when present in so large quantities as to interfere with natural secretions; but, as it condenses from air, at any usual temperatures in the habitable part of the globe, until the quantity of water present cannot exceed four to six per cent., the danger of such interference is almost entirely removed.

Having discussed the constituting elements of *air* and their characteristics as chemical bodies, some of the physical properties which bear important relations to ventilation and heating may next be noticed. The three conditions of material substances are gaseous, liquid and solid. An ideal perfect gas is perfectly fluid and perfectly expansible or compressible; relief of pressure or the addition of heat, with permission to expand under the same pressure will cause an indefinite enlargement of volume proportionate to the pressure or heat, while increase of pressure or abstraction of heat under constant pres-

sure produces proportionate reductions of volume also indefinite in amount. Although the discoveries of the past three years have rendered it nearly certain that no gaseous body whatever exists which at some pressure or temperature does not lose its gaseous form and become liquid, yet within the range of temperature and pressure of nature on the surface of the earth, the air may be treated as conforming to the ideal laws of a perfect gas.

It is not the less a material substance that *air* is a gaseous body. Its weight for a given volume, under a given pressure and at a given temperature, is well known. Thus at the pressure of 14·7 pounds upon a square inch and at a temperature of 32°F., one cubic foot weighs 0·0807 pound; or 12·4 cubic feet weigh one pound. Now, this pressure of 14·7 pounds on a square inch is the atmospheric pressure found to exist on the surface of the earth, and is the equivalent to 2116·3 pounds on a square foot. If 12·4 cubic feet weigh one pound, it would take a column of air of 26,227 feet to exert the load of 2116·3 pounds on the square foot, or very nearly five miles high. But the air changes in density as the pressure is reduced; or, in other words, as weight of the column of air becomes less and less towards the top, the volume of each cubic foot increases, so that the atmosphere is really 40 to 45 miles in height in place of the 5 miles which would exist if it had a uniform density.

The pressure of air is measured by the barometer, and 14·7 pounds to the square inch corresponds to 29·92 inches of a column of mercury in the mercurial barometer. This instrument may be briefly described as a glass tube about three feet in length, with one end closed, which tube having been filled with mercury when the closed end was downwards, is reversed into a shallow cup also holding mercury; when the column in the tube will leave the upper or closed end (and form a vacuous space) and descend into the cup, so far as the pressure of the air on the surface of the mercury in the cup will not support the column. There is found to exist at any place not much elevated above the level of the sea from 28 to 31 inches of length of this column; or, in other words, of difference in height of surface of the mercury near the top of the tube and that of the open cup at its foot. These three inches of variation of barometrical height are the limits of usual variation of atmospheric pressure.

The volume of air under any given pressure is much affected by heat. In common with most gases (and probably of all where the

temperature of the gas is considerably above the point of liquefaction), air expands or contracts $\frac{1}{491}$ part of its volume for each degree (Fahrenheit) of temperature above or below the freezing point. This change of volume is the great natural agent in promoting that circulation of the air and distribution of the heat from the sun which makes our globe habitable. A correct appreciation of its effect upon the air may be had by examination of the following table, which includes only a few usual atmospheric temperatures. The same laws, with small modifications, govern the volumes and densities of air to the highest temperature of combustion:

Temperature of dry air, degrees Farenheit	0°	10°	20°	32°	45°	50°	60°	70°	80°	100°
Volume of same weight, of air	459	469	479	491	499	509	519	529	539	549
under the same pressure	0·935	0·955	0·976	1	1·016	1·037	1·057	1·077	1·098	1·138
Density for constant volume	1·070	1·047	1·025	1	0·984	0·965	0·946	0·928	0·911	0·878
Weight per cubic foot-pounds	0·0863	0·0847	0·0828	0·0807	0·0794	0·0779	0·0765	0·0749	0·0735	0·0709

If, however, the pressure upon any given volume of air becomes greater or less, its temperature will then be found to have increased for the greater pressure and to have diminished for the less pressure. It results from this that the air upon the top of mountains, where the barometric pressure is greatly reduced, is found to be much colder than at their feet, until at the elevation of from four to five miles above the elevation of the sea a region of perpetual frost, even in the torrid zone, is reached.

There are two recognized standards of measurement of heat of substances. The first is that of the intensity or temperature. All bodies of unequal temperatures possess a tendency to equalize their temperatures by transfer of heat between themselves, when such bodies are either in actual contact (in which case the process is called conduction or convection), and also when they are in some degree in proximity to each other (in which latter case the process is denominated radiation), some, if not all, of the heat rays being found to pass through most gases and through some solid bodies. Such gases or bodies are denominated diathermanous. As substances generally expand by the increase of their heat and contract with its decrease, the extent of this

change of dimension between certain temperatures determined by natural phenomena has been used as a measure. The phenomena referred to are the freezing and boiling of water, and the temperatures communicated to thermometers (heat measures) by water at the freezing or boiling point (under defined atmospheric conditions) established limits for a range of expansion, which range is divided into parts called degrees. Three scales of division have had practical use, but one of them (Reaumur's) of 80 parts may be considered as superseded at this time. The other two are: first Fahrenheit's, where 180° of equal expansion are made between freezing and boiling, and where the freezing point is called 32°, and the same rate of equal expansion (contraction in this case) is carried downward below the freezing point to an imaginary zero; bringing the boiling point $32° + 180° = 212°$ above zero; and the second, Centigrade, where the freezing point is called zero, and 100° are spaced off from zero to the boiling point. By the English speaking nations the Fahrenheit scale is used as a popular scale almost altogether, and to a great extent as the scientific one. In other countries the Centigrade scale is in general as well as scientific use, and the next fifty years will probably witness its universal adoption in all countries. The mercurial thermometer is too well known to need description. The principle of measurement of temperature by the expansion of a body by heat is extended above the boiling point by degrees of supposable equal values to 10,000°, 15,000°, 18,000°, the last being the theoretic heat of carbon burning in oxygen, and is carried below the freezing point in the same way to the lowest temperature of existence in nature, and *to the utmost cold supposed to be possible.* The contraction of gases by removal of heat of one degree Fahrenheit, was stated to be $\frac{1}{491}$ part of the volume at 32°. Now, if it be imagined that the contraction were carried on for 491°, the gas must obviously disappear at the next diminution. This imaginary temperature of $32° - 491° = -459°$F., has been deemed the zero of absolute temperature, and it has been found that by adopting this supposed value in computations, the laws of expansion and elasticity of air, or gases, together with those of accompanying heat, can be expressed satisfactorily.

The second standard of measurement of conditions of heat refers to the quantity of heat which may be taken up or given out in effecting changes of temperature in the various substances. For this purpose water is again selected as the means for establishing a thermal or heat

unit. The English heat unit is taken as the heat appertaining to one pound of water heated one degree Fahrenheit. The foreign and scientific heat unit is the heat belonging to one kilogram of water heated one degree Centigrade, and is 3·97 times that of an English heat unit; but the English heat unit continues to be used in most treatises on applications of heat in the English language, and will be the only one referred to in this paper.

The specific heat of a substance is that quantity of heat, expressed in heat units, which must be transferred to or from a pound of that substance to effect a change of temperature of one degree. The quantity varies greatly for different bodies, and varies also in some measure at the different points in the scale of temperature in most of them. In the latter regard, however, the variation is so small that we can accept certain values which have been ascertained by experiment, and will be found in tables of specific heats of substances in books on physics as sufficiently accurate for practical purposes. For gaseous bodies the uniformity of specific heat at different points in the scale of temperature is more closely preserved than for solids or liquids, but these bodies are found to have *two values* for specific heats: one for the increase of temperature of one pound of gas, one degree, where the gas is permitted to expand under constant pressure; and the other, where the gas is enclosed so that, in place of expanding, the pressure increases in accordance with a certain law of elastic force dependent upon the addition of heat. Thus the specific heat of air, under constant pressure, is 0·238 heat unit; that is, 0·238 pound of water, losing one degree of heat, will impart to one pound of air (about 13⅓ cubic feet at 70°) one degree of heat, while the volume of the air will have increased, under constant pressure, $\frac{1}{3\frac{1}{29}}$th part. On the other hand, the specific heat of air, with constant volume, is 0·169 heat unit only; one pound of the air retained (to 13⅓ cubic feet in the supposed case of 70°) in its original volume will be heated one degree by 0·169 pound of water losing one degree. This value of specific heat for constant volume does not apply to all gases, although nitrogen, oxygen, hydrogen nearly conform to the figures given.

The specific heat usually quoted and applicable to the theory of heating and ventilation is that of constant pressure, or 0·238 heat unit for air.

To complete this statement of the effects of heat, latent heat must be mentioned. Whenever any material substance passes from one of

its three conditions—gaseous, liquid or solid— to another, heat units are absorbed or given out, often in enormous quantity, without any apparent change of temperature of the transformed substances. Thus water at 212° requires the addition of 966 heat units to transform it into steam of 212°. Evolution or absorption of latent heat also accompanies chemical combinations.

One more property of heat should be named. Heat is a measure of force expended or utilized, and one heat unit represents the force of lifting 772 pounds to the height of one foot = 722 foot pounds.

Resuming the consideration of the physical properties of air.

The beginning of this paper mentions that *very* small quantities of various substances which cannot be considered as gaseous bodies were to be found in the air of habited or habitable places. Generally, especially in towns or cities of the temperate regions where manufacturing callings or the comfort of the inhabitants demand the use of fires, the main portion of these impurities of air consist of dust of minerals, and metals—smoke (which is principally dust of charcoal or mineral coal), and similar bodies reduced to so fine a state of powder that the adhesion of air to the particles prevents their settling in it, except so slowly that they may be said to float, and as floating bodies will have been dispersed with the currents; and they may be in some measure diffused by the inter-currents of diffusion of vapor of water or carbonic acid or other gaseous bodies with which they were particularly associated in their origin. The effect of this adhesion of air to particles of matter can be appreciated by stating that, but for it, rain drops would reach the ground with an acquired velocity equal to that of shot from a gun; and by it the impact of hailstones, even of the largest size, is modified so far as to reduce the injury they occasion, to the destruction of glass, defoliation of trees, and similar results, such as might happen from stones thrown by the hand. In the case of a hail storm the phenomenon is produced by a violent ascending current of air, which at the height of five to eight miles reaches the region of perpetual frost, where the hailstones are formed, and the stones descend through and against a current of from 20 to possibly 200 miles per hour.

The dust referred to, after all, except in localities where decidedly injurious fumes are generated, or in workshops, or any business where a volume of dust or smoke is created, or the exposure of the workman to breathing it, is improperly guarded against, can only be considered

rather as objectionable than noxious. This question of means of prevention or removal of dust or smoke will come up again when considering the practical applications of ventilation.

Odors or smells constitute palpable impurities—vitiations or, perhaps, quite harmless portions of the air. Some of them are unquestionably dusts of solid bodies; others are definite chemical gaseous bodies; others, again, are seemingly in combination with the vapor of water, in which they are dissolved in the atmosphere. Dust of organic matter; small particles of the skin, and fatty matters detached from the skin are abundant in the air of all houses; in the air of the streets similar exhalations from horses and other animals can be detected; in the air of the country, vegetable particles predominate. These dusts are in every stage of decomposition. When first separated from their source they are generally undecomposed and inodorous, but sometimes, like pollen, they possess the power of affecting the sense of smell; in moist atmospheres they rapidly decompose, having become the soil for numerous microscopic growths which accompany, and it is now satisfactorily determined, occasion the decompositions.

It must be borne in mind that the *quantity* of organic matter described so briefly in the preceding paragraph is exceedingly diminutive as compared with the volume of air. Light as air is, not the one hundred millionth part of its volume for pure air on high ground, or about one five millionth part in a crowded railway carriage (as deduced from figures of Dr. Angus Smith), is organic impurities. Yet to a very small portion of this very small portion of the air is now attributed, by the best authorities, the greatest danger from breathing of vitiated air.

It has been long known that fermentation or some similar action accompanies most, if not all, organic decompositions; and it was reserved to Drs. Schrœder and Pasteur, especially to the researches of the latter to demonstrate that countless germs of vegetables and infusoria exist in the air, which will develop wherever suitable organic matter is found to support their growth. These views were combated by several writers, some of whom, admitting the fermentative growths, supposed or advocated their origin by spontaneous generation. But this last view has now been satisfactorily refuted by many experimenters; Prof. Tyndall's investigations being very conclusive in showing that with pure air no change of the most decomposable substances and solutions commences. Prof. Tyndall's test for the purity of air is to

allow a sunbeam to shine down a tube containing air which has been filtered through cotton-wool. The smallest particles of dust, germs or grains beyond the power of discernment by the microscope, are illuminated by this test to brilliant points, until their presence becomes evident to the observer.

It is positively known that different chemical changes are brought about by different germs. Alcoholic or acetous (vinegar) fermentation, lactic acid, butyric acid, etc., proceed from different vegetable or organic growths. It finally seems probable that the whole train of epidemic diseases owe their origin to atmospheric germs which find their suitable organic matter for growth in the human system.

The organisms themselves are minute almost beyond the limits of conception. One of the most common, the Bacterian termo, which is a living organism, "has a wasp-formed body, each enlarged part being about $\frac{1}{14000}$ of an inch long and $\frac{1}{45000}$ of an inch wide, joined by a filament of extreme thinness, about $\frac{1}{14000}$ of an inch long; and has a 'flagella' $\frac{1}{7000}$ of an inch long at each end, not over $\frac{1}{1000000}$ in width, and so thin as to be undiscernible in the side view. The flagella is lashed incessantly." What must be the magnitude of the germs of this perfect organism? Beside the germs of the Bacteria, those of Vibrios, Mycodermes, Mucidines and Torulæ are given by Pasteur and others, as of known organisms whose characteristics and purposes are well established. The almost immediate occurrence of fermentation of some particular kind in each fermentable liquor when exposed to common air in any place, and the complete suspension for an indefinite time of fermentation when only *pure* air, tested as Tyndall has described, comes in contact with such liquor, must be accepted as proof of the agency and universality of atmospheric germs. Heat far beyond the boiling point will not kill certain of these germs, and some defy certain chemical agents which are destructive of life generally.

Each new consideration of the subject of the effect of air on the health of mankind, adds more evidence or reasoning to support the view that gaseous impurities are fraught with but a very small portion of the danger which appertains to the organic vitiations.

"It should be here remarked that the basis of the *germ theory* is, that all natural decompositions of organic substances (in contradistinction from decompositions, by fire or chemical action) are growths. Growths in themselves healthful of their kind (same as the lion or the mosquito

have healthful lives—the oak or the weed have healthful growths). Each growth, it may be descending in the scale of growth and ascending in another path, to be again reduced. A cycle of vitality from the lowest organism through the vegetable and animal life to man, and returning by a retrograde course (if it be retrograde) to a new train of existence."—[Excerpt from a report on the sanitary condition of a building, by the writer.]

The next step in the course of the investigation is to consider the quantities of air requisite for the health and comfort of the human occupant of the ventilated room or place, for a given interval of time. These quantities are composed of several distinct requirements, the most essential of which are the *necessities* of respiration and of absorption of the vapors of transpiration; inadequate supply of air for these purposes resulting in suffocation. Another requirement for dwellings, is the supply of air for fuel or for lighting, in the latter case for the dispersal of heat in some degree. But all these requirements, essential as they are, call for quantities of fresh air, quite insignificant in amount to the demands for dilution of the products of respiration, transpiration and combustion, so that the air of a dwelling shall have that degree of purity which is conducive to healthful residence.

The healthy adult man, in still life, in an atmosphere of normal condition (which, in our climate, may be taken at the temperature of 60° to 70°, with 80 to 70 per cent. of humidity), whether awake or asleep, inspires on an average, 30 cubic inches and expires a corresponding quantity, slightly dilated by heat and by chemical change at each respiration, and he breathes 16 times each minute; giving 480 cubic inches (or 0·278 cubic feet) of air demanded each minute. Let it be supposed that the air inhaled in pure air with 0·0004 its volume of carbonic acid gas, and to have the temperature of 70° with 70 per cent. of humidity; in such case, the exhaled breath will have a temperature of 90°; and the entire volume will have been increased about $7\frac{3}{100}$ per cent.; it will be saturated with moisture, which will form over $4\frac{7}{100}$ per cent. of its volume; while the increase of weight, by taking up moisture and carbon from the lungs, will have been only $3\frac{6}{10}$ per cent.; at the same time, from the 20° rise of temperature, the density, as a whole, will have been reduced $3\frac{2}{10}$ per cent.; the carbonic acid gas in exhaled air will be $3\frac{8}{10}$ per cent. of its volume; and the quantity of oxygen of the inhaled air will have been reduced about $19\frac{3}{4}$ per cent.

The average respirations of an adult for one minute will tabulate as follows:

Table of constituents and products of 16 *respirations, of* 30 *cubic inches each.*

Constituent gases.	Inhalations at 70° temperature and 70 per cent. humidity. Wt. = 0·07443 lbs. per cubic foot.			Exhalations at 90° temperature, saturated with aqueous vapor. Wt. = 0·07204 lbs. per cubic foot.			Ratio of exhalations to inhalations.	Changes effected by the act of respiration.
	Wts. lbs.	Vols. c. ft.	Vols. per ct.	Wts. lbs.	† Vols. cu. ft.	Vols. per ct.	Vols. per ct.	Wts. lbs.
Nitrogen.....	0·015898	0·2181	0·7854	0·015898	0·2264	0·7615	0·0381	0
Oxygen.....	0·004542	0·0543	0·1973	0·003645	0·0456	0·1534	0·3839	—0·000897
Aqueous vapor.....	0·000221	0·0047	0·0169	0·000629	0·0140	0·0471	0·9787	+0·000408
Carbonic acid.....	0·000013	0·0011	0·0004	0·001246*	0·0113	0·0380	102·73	+0·001233
Total.....	0·020674	0·27771	1·0000	0·021418	0·2973	1·0000	1·0703	+0·000744

The difference of the changes in weight of carbonic acid and oxygen in the last column of the table = 0·001233 — 0·000897 = 0·000336 lb., represents the weight of carbon taken from the lungs, being an actual combustion of so much carbon in the system (as appears from the breath) each minute.

To the casual reader it would appear that the computations of the above table were needlessly extended, especially when it is considered that the data are assumed averages, and not absolutely accurate; but the fact is, that the vitiations of air bear so small a part in relation to its volume, that it requires long lines of decimals to express them at all; and accuracy in the last figures is demanded, to make the proportions of what are admitted vitiations, or rather are admitted as the accompaniment or vehicle of vitiations, appreciable.

Beside the air needed for respiration, an uncertain quantity is both

* The quantity of carbonic acid exhaled, adopted in this table, is deduced from the experiments of Dr. Edward Smith, Proc. Roy. Soc., 1859.

† This column of vols. cu. ft. was obtained thus:

	Weights. Lbs.	Weight per cu. ft. at 70°. Lbs.	Cu. ft. at 70°.	Cu. ft. at 90° = 549-529ths = 1·038.
N_2	0·015998	0·0729	0·2181	0·2264
O_2	0·003645	0·0824	0·0439	0·0456
H_2O,	0·000629	0·0466	0·0135	0·0140
CO_2,	0·001246	0·1142	0·0109	0·0113
	0·021418		0·2864	0·2973

needed and vitiated by transpiration. A constant exhalation of carbonic acid gas transpires from the skin; by no means so large in quantity as is emitted with the breath, but probably one-fourth or one-fifth as great. The regularity of transpiration nearly equals that of respiration. Accompanying this, it is probable that an absorption of oxygen, corresponding to the equivalent of oxygen in the carbonic acid, takes place. The best authorities do not seem to have found the expired air from the lungs to have lost more oxygen than the carbonic acid exhaled required; and as all authorities assert the exhalation of carbonic acid from the skin, it follows of course that the supply of oxygen to form this carbonic acid must be absorbed by it. The phenomenon of interchange of gases occurs with the cutaneous secretions, similarly, if not equal in extent, to what happens in the so-called revivification of the blood.

The exhalation of moisture from the skin, however, is a very variable quantity as compared to what exhales from the lungs. The internal temperature of the human being is perhaps 98°, while the comfortable and healthful temperature of the air in contact with the skin is from 10° to 30° below this point; the degrees of heat of the air, varying greatly with its hygrometric condition—or, in other words, with the proportion of moisture present. The loss of heat from the evaporation of moisture from the skin into the air, being far greater than the cooling effect of the air itself. In temperate regions, also, a large part of the person is protected by clothing, whereby the temperature of the air next the skin, under the clothing, is elevated, until, for instance in our climate, an admitted summer temperature of 70°, accompanied by 70 per cent. of humidity, is the standard of condition for the active man, although a higher rate of humidity (80 per cent.) is perhaps more conducive to luxurious comfort and ease.

Be this temperature and corresponding moisture condition what it may, the fact remains that by insensible perspiration, as it is called, a large amount of moisture is exhaled from every human being each day, hour or minute, and this moisture is laden with organic matter; and a certain quantity of fresh air is needed to absorb and dilute it, and the accompanying organic vitiations.

Some observers, after considering the relative quantities of liquids and solids taken as food and excreted daily, have estimated that from 1·5 to 2·5 pounds of liquid, must, on an average, be dissipated from the system of an adult in active life in the time named. The mean of

these quantities may be accepted as the loss by evaporation from the lungs and skin in occupied places = 2 pounds; when about 0·0014 pound will pass from the skin and 0·0004 pound is evaporated from the lungs each minute. On the other hand, the exhalation of carbonic acid, as before stated, is not nearly as much, probably, from the skin as from the lungs.

It should also be stated that a small quantity of nitrogen has been found to be absorbed by the lungs, and a very little ammonia is either given off, or is formed almost instantly, by decomposition of some of the emitted organic matter. These vitiations are, however, only appreciable by delicate observations, which observations have given such discordant results as to throw doubt on the experiments as bases of theory. Still it may be asserted that nitrogen is the natural and fundamental part of the atmosphere for the types of animal life on the face of the earth, and that no other gaseous body can be admitted to replace it; while oxygen, in the proportion in which it is always found in the air, is the necessary and sole active agent in sustaining life.

From all that has been said in this article, it will be evident that, for the purpose of breathing solely, only a little more than one-quarter of a cubic foot of air is needed each minute by the average healthy adult. Perhaps this quantity will be raised to one-third of a cubic foot when the air of transpiration is included. And that there must flow away from the person, by respiration and transpiration combined, also each minute, 0·0014 pound or 0·03 cubic foot of vapor of water at 70°. If these quantities of exhalations, small as they are, are positively and absolutely removed, and fresh air substituted for the first of them, a perfect ventilation will have ensued.

The sole mode of removal possible, is by diffusion and dilution. The purity of air in any occupied place can only be relative. A certain quantity of exhalations in a given time will mingle with a certain quantity of fresh air supplied in that time (neglecting the loss by transfusion through walls, as septa, of some small quantities of carbonic acid and vapor of water, the latter especially when the exterior dew-point is low); and a definite ratio of the constituent parts of the air of any occupied place, will eventually be established.

It is customary to attempt the establishment of the proper quantity of fresh air by the ratio or percentage of carbonic acid *admissible* in a habited room. If it is supposed that twice the quantity of carbonic

acid is admissible in a continuously occupied room over that existing out-of-doors in fresh air, then 99·1 times as much fresh air as is needed for respiration, etc., must be supplied to dilute the exhaled air, a proportion which gives 33 cubic feet of air to each person per minute, with a result of 0·0008 volume of carbonic acid present.* The quantity of carbonic acid in any closed room will be further reduced by some diffusion at cracks of doors or windows.

Another method for determining the quantity of air needed is based on the diffusion of vapor of water. The supposition that the hygrometric condition of the air is to be elevated, say 5 per cent., will, if the temperature of the air of the room is 70°, allow the diffusion of 0·000056 pound of vapor per cubic foot of air, or for the 0·0014 pound of vapor emitted from the person each minute, 25 cubic feet of air to each person per minute.

Either of the above ways for *computing* the requirements of ventilation are purely empirical and founded on no reasonable or natural demand. The quantities they give, however, are about those adopted by the best authorities as the least for healthy persons, while double these quantities are required in hospitals. The air of dwellings and of hospitals has *proved* to be pure to the sense of smell with the quantities above stated, if the distribution and removal has been well arranged; with less quantities this is not the case. After all, the standard of purity of air is founded on the *perception* of an almost infinitesimal quantity of organic matter, and upon results of tests of health of dwellings, etc., and not upon the reasoning of the chemist.

Following the requirments of defined quantities of air for personal ventilation of the inhabitants of rooms, further demands for the purposes of supply of air to fuel used at times in heating them, and for the consumption of gas, oil or other material producing light by burning, should be investigated. What is needed for warming, however, may be more properly considered when discussing the heating of dwellings or other places, only remarking here that the quantity relative to what is requisite for *dilution* of the air of breathing, or for

* These figures are obtained as follows: Accepting the air for respiration at 0·2777 cubic foot at 70° per minute, and adding one-fifth for one for transpiration, we have 0·3331 cubic foot per minute. With the volume of air which shall give the requisite excess of 0·0004 CO_2 added to the normal air, the effect of raise of temperature may be neglected, when the ratio of volume of CO_2 in the exhalations to that in the inhalations becomes 1 to 99·1, in place of 1 to 102·7, which was the ratio for an increase of temperature of 20° (70° to 90°) $\frac{1000}{11} = 90{\cdot}1 \times 0{\cdot}333 = 33{\cdot}0 + 0{\cdot}333 = 33{\cdot}33$ cu. ft.

reduction of heat, is so small (while the diluted or vitiated air has sufficient oxygen not to be impaired for supporting combustion of fuel), that it drops out of consideration in the question of volumes of air to be furnished. And there is left for consideration at this time only what supplies of air are requisite for gas burners and oil or other lights.

The gas burner in common use will burn from three to six cubic feet of ordinary coal gas per hour; each cubic foot of such gas consumed will take up the oxygen of 6·1 cubic feet of air, and will require the presence of about 12·2 cubic feet of air at the point of ignition, in order to effect complete combustion. This double supply of air is found in practice necessary for the combustion of fuel of all kinds, under usual conditions, in air of usual temperatures, and the escaping gases, when burning hydrogen or carbon, will consist of vapor of water and carbonic acid (if the carbon is entirely burned), as novel chemical products, together with free nitrogen, and as much free oxygen as was not taken up in the chemical combinations. In the same way it was noticed that the air expired in breathing had been deprived of only about one-fifth of its original oxygen, and it was then accepted that such expired air was unsuitable for a new respiration.

The average gas burner in general use may be assumed to burn 4½ cubic feet of gas per hour, and the quantities reduced to the unit of a minute, so as to be comparable with the estimate for respiration as previously established, give 0·075 cubic foot of gas, which takes up, by chemical combination, the oxygen of 0·46 cubic foot of air, and needs 0·02 cubic foot of air to accomplish the burning. The products of combustion, together with and including the free nitrogen and oxygen, forming the *gases of combustion*, have the volume of 0·97 cubic foot, when reduced to the temperature of 70°, at which temperature all the foregoing figures have been taken.

Thus it is seen that the demand of air for a gas burner (burning 4½ cubic feet per hour) is very nearly three times as great as that for the respiration and transpiration of an adult man in still life. If, however, the same rules for determining the quantity of air for dilution of the carbonic acid or vapor of water generated, are applied to gas burning as were used in the case of respiration, we have the following results. A 4½-foot burner will generate 0·0455 cubic foot of carbonic acid each minute, whence 114 cubic feet of fresh air will be needed in the same time to dilute this carbonic acid so that in the

resulting mixture 0·0008 volume of carbonic acid (twice the normal quantity) will be present. The same burner will produce 0·00475 pound of vapor of water each minute, which calls for 85 cubic feet of air, if the condition of adding 5 per cent. to the humidity at 70° is thought to be the standard for attainment.

In the act of respiration, as discussed in the last number, it was shown that 0·000336 pound of carbon was consumed in the system, as measured by the expirations, each minute. The estimate of this quantity is increased by some emitted carbonic acid by transpiration; adding, as before assumed, one-fifth, then 0·0004 pound of carbon can be accepted as consumed each minute. Whence, recognizing that 14,500 units of heat proceed from the perfect combustion of carbon into carbonic acid, it results that 5·8 units of heat will be produced.

From this quantity of heat is to be deducted the heat requisite to vaporize the exhaled moisture from the lungs and from the skin. The previous assumption of vapor emitted each minute, at 0·0014 pound, multiplied by 1062° (= the latent heat of vapor at 70°), gives 1·49 units of heat as absorbed in the evaporation, leaving 4·31 units of heat to be accounted for. What proportion of this heat is taken up by the labor of work, or in the functional demands of animal or mental life is very uncertain.

Taking the 30 cubic feet of air allotted in the last number for the requisite of ample ventilation of a person each minute, we have 30 × 0·0744 (the weight of one cubic foot of air, of 70 per cent. humidity, at 70° temperature) = 2·232 pounds of air at 70°; multiplying by 0·238, or the *specific heat* of air, we have 0·531 as the number of heat units demanded to heat the 30 cubic feet of air 1°. If it be assumed that all the heat unaccounted for—the 4·31 units—is expended in heating the 30 cubic feet, then the temperature of the 30 feet will be elevated a little more than 8°. It is not probable, however, that the amount of heat to be dissipated exceeds one-half the total, and possibly one-third is nearer the case. I think that the elevation of temperature of the surrounding air, when 30 cubic feet of air per minute is allotted to each adult in still life, does not exceed 3°, but am ready to admit that the grounds for this belief are too nearly a mere guess to be satisfactorily stated.

It is not unfrequent that in a crowded room, in warm weather, a number of persons will be collected together who will have been provided with not over 10 cubic feet of air per minute. On the supposi-

tion above, the temperature of such a room would be raised from 70° to 79°, presenting some probability of coincidence with facts. But the effect of any such elevation of temperature with the supposed limited supply of air, will be to increase the avidity of the air for moisture and to promote perspiration, which will again afford relief by the evaporation of water, and thus limit the proportion of heat given to the surrouding air to temperatures of endurance. As the temperature of air rises, the amount of heat given out by evaporation will increase until it even exceeds that given out by conduction or radiation, or by both combined.

The heat effects from a gas burner are as follows: taking the gas as having the usual quality of 14 to 15 candle power, the heat proceeding from such gas is very nearly = 622 units for each cubic foot of gas burned. [Gas of 14 to 15 candles is such that when 5 cubic feet are burned in a properly shaped burner, under ½ inch water column pressure, in one hour, the light given out will be equal to that proceeding from 14 or 15 standard spermaceti candles, each of which shall burn at the rate of 120 grains of spermaceti per hour.] This gives the hourly heat production from 4½ cubic feet to equal 2800 units, or 46·7 units to be dispensed each minute. The existence of this quantity of heat in combination with the gases of combustion as they arise from the flame, is one of the best established facts in physics, but its dispersal when these gases are diffused is scarcely reconcilable with the observed heat imparted to a closed room by a gas burner. The quantity of heat which will have disappeared by the diffusion is indeterminate. I am not now willing to admit that over one-third the heat which has been theoretically evolved, will have been imparted to the air of a room.

It has been customary to assume that 10 cubic feet of air per minute for each cubic foot of gas burned per hour should be supplied for *ventilation* of a gas burner. This rule gives 45 cubic feet of air for a 4½ foot burner. From such a burner, under such circumstances, the temperature of the air of the room being 70°, that of the air ascending from open burners will be 99° and that ascending from argand burners will be 128°, *on the supposition that none of the heat is wasted in diffusion.* Supposing only one-third of the heat to be imparted to the gases or air of dilution, the temperature of the ascending currents become 79½° and 89° respectively. The real temperature of the gases

rising from the flame of a gas burner, unmixed with air of dilution, may be stated at 2640°. Unless enclosed in a chimney of some kind, these gases rapidly mix with the air around them, until within two or three feet they fall generally below the boiling point. Still the current which reaches the ceiling of a room is generally much elevated in temperature, and spreads over the surface as a stratum, with little tendency to descend or to mix downwards, except by diffusion. This fact and the comparatively brief time of gas lighting, are great aids in meeting the difficulty from heating and from gases of lighting. Another thing must be borne in mind, that there are no organic impurities to be dispersed from the products of combustion. Discomfort, and in extreme cases even suffocation, may follow the want of ventilation of burners, candles or lamps, but disease, in a strict sense, cannot arise from this cause. It is clear that the test of proportion of carbonic acid present, as a measure of vitiation, does not apply to lighted rooms.

It must be noticed that the rate of supply of air for gas burning *i. e.*, 45 cubic feet per minute for a $4\frac{1}{2}$ foot burner, will eventually bring up the rates of carbonic acid in any room (in course of time, however large the room may be) to 0·0014 volume, supposing the normal fresh air to have 0·0004 volume in it, and supposing that no diffusion of carbonic acid occurs through walls or cracks, where air will not circulate as a current.

The ventilation of candles or lamps could be investigated with equal care to that which has been given to gas lights, but it is sufficient to say here that the average candle gives about one-fifteenth the light proceeding from an average gas burner, and about equals the ordinary hand lamp, with oil as the burning material. Either of these can be taken to have somewhat greater heat effects than gas of the same luminous value, and about 5 cubic feet of air per minute can be taken as the quantity needed for each candle or lamp. The carcel or petroleum argand lamps can be estimated at slightly less heat effect for given light production, and the smaller ones will equal two-thirds an average gas burner in this regard; such lamps will require 30 cubic feet of air per minute.

Applications of Electricity.—It has been sometimes thought that a copper cable of enormous thickness would be required to transmit the hydraulic power of Niagara falls to New York. Prof. Ayrton has shown that the whole power could be transmitted by a slender copper wire, provided that the wire could be thoroughly insulated. He has also shown that the only hindrance to receiving the whole power is the mechanical friction of the machines. It is therefore believed that immense machines, with continuous currents, with detached exciters or magneto-electric machines, driven very rapidly by steam engines, will hold an important place in the future transmission of energy. With such machines it would be possible to warm, to light and to give workshops the power which is necessary to move all their machinery by means of an ordinary telegraph wire, thoroughly insulated and transmitting energy from great distances. Prof. Perry thinks that it will sometime become possible to see what is going on in remote places by means of electricity.—*La Lumiere Electrique.* C.

Decomposition of Nitrates by Decomposition in the Dark.—In plants which grow in the dark, although there is a regular organization and development of stalks, roots and appendages, there is a persistent elimination of a portion of the material which was contained in the seed. The tissue of the organs which are developed under a shelter from light is firm and strongly impregnated with a liquid which has a sensibly acid reaction. The cotyledons are, therefore, provided with the principles which are necessary for the life of the embryo; but nocturnal vegetation is unable to fix the carbon of the carbonic acid which is in the air. Boussingault has experimented in order to find whether this impotence extends to the fertilizing substances, which the roots commonly draw from the soil; if, for example, nitrogenous compounds, such as the nitrates and the ammoniacal salts, are assimilated. He immediately found that if a measured quantity of these salts is added to a sterile soil a considerable portion disappears entirely, and only a part of the quantity is found in the growth. He therefore concludes that a soil which had been rendered entirely sterile contains traces of organic substances after vegetation in the dark. These substances are probably due to an excretion from the root, which exercises a destructive action upon the acid of the nitrates that were added to the soil.—*Ann. de Chim. et de Phys.* C.

Phosphor-Tin.—An alloy of tin with phosphorus is much used in Germany, especially in the preparation of phosphor-bronze. The mixture should contain at least 9 per cent. of phosphorus in order to secure a thorough incorporation of the tin. If more than 9 per cent. is introduced the excess oxidizes and is driven off by the heat. A mixture which contains 9½ per cent. of phosphorus satisfies the formula P_2Sn_5, corresponding to the oxide, P_2O_5.—*Der Techniker.* C.

Relations of Intensity in the Sodium Lines.—Chase's suggestion of a probable planetary influence upon spectral lines has been partially confirmed by the investigations of Dietrich upon the comparative intensity of the two sodium lines. Experiments upon different days showed differences of intensity, which furnish some indications of regular progression, and which show, in the opinion of the experimenter, that the lines do not consist of homogeneous light, but of beams of slightly different wave lengths. He proposes to continue the investigations for different flames, and to publish the results at no distant period. The variability which Lockyer first noticed was most strikingly indicated in the components of the sodium line.—*Wiedemann's Annalen.* C.

The Brin Process for Oxygen.—Boussingault first discovered the property of barytes to absorb the oxygen of the air at a certain temperature and to restore it at a higher temperature. Great practical difficulties have arisen, from the fact that the barytes rapidly become inert and require to be revived, and that the oxygen is usually very impure. Messrs. Brin Bros. heat the commercial sulphate of barytes, in special furnaces, with 25 per cent. of carbon, in order to form a sulphuret, which is dissolved in water and treated by nitric acid. A nitrate is thus obtained which, when calcined in a special furnace, gives a caustic barytes at a cost of 2½ francs per kilogramme (19 cents per pound). Then, by a proper preparation of the air in order to render it easily decomposable, by the use of pumps and ventilators or aspirators in order to facilitate the peroxidation of the barytes and extract the oxygen afterwards, and by the employment of special pyrometers for the automatic regulation of the furnace temperature, they are already able to produce oxygen, of great purity, at a cost of 62 centimes per cubic metre ($3.50 per 1000 feet). They think that by manufacturing upon a large scale the cost could be reduced to about one-fifth of this amount.—*Chron. Indust.* C.

Bronzing Copper.—At the Paris mint medals are bronzed by boiling them for a quarter of an hour in the following solution: Pulverized verdigris, 500 grammes; pulverized sal ammoniac, 475 grammes; strong vinegar, 160 grammes; water, 2 litres. An untinned copper boiler is used, and the medals are separated from each other by bits of glass or wood.—*Les Mondes.* C.

Chinese Acoustics.—A Chinese physicist asserts that the law, which is commonly accepted, and which states that the octave of any note may be produced by doubling the length of a musical cord or tube, is strictly true only for cords. He says that experiments with tubes of different lengths and diameters have led him to the conclusion that the ratio of length is as 4 to 9, instead of 1 to 2. The interest of the Chinese in foreign science is shown by the publication of numerous English and American scientific treatises in the Chinese language.—*Les Mondes.* C.

Conservation of Electricity.—Quantity of matter and quantity of energy are not the only magnitudes which remain invariable. Lippmann claims that quantity of electricity enjoys the same property. If we study any electric phenomenon thoroughly we find that the distribution of electricity may change, but that the sum of the quantities of free electricity never varies. If the electric charge undergoes a positive variation at certain points there is a corresponding negative variation at others, and the algebraic sum of all simultaneous variations is always zero. This law constitutes the principle of conservation of electricity.—*Comptes Rendus.* C.

Inverse Electro-motive Force.—LeRoux has published a simple process for showing an inverse electro-motive force in the voltaic arc. It consists in extinguishing the arc by opening the circuit and immediately re-establishing, by hand, the communication between the two carbons through a galvanometer. This shows the existence of a current going from the negative to the positive pole between the heated carbon points and in the contrary direction in the galvanometer. Hence arises the difficulty of illuminating two or more arcs in a continuous current, since it is necessary to overcome the same inverse force for each arc. Magneto-electric machines with alternating currents profit by the existence of this inverse current, and this is one source of their great advantage over ordinary batteries.—*Comptes Rendus.* C.

Magnetic Intensity of Iron.—According to Rinman's investigations the magnetic intensity of iron appears to increase with the increase of carbon. In three specimens of cast iron containing ·09, ·30 and ·57 per cent. of carbon the magnetic intensity was 5·5, 9·5 and 11, respectively.—*Berg- und Hütten-Zeitung.* C.

Velocity of Light in Quartz.—In investigations upon refraction, Kohlrausch found nearly uniform values in different pieces of any given substance, while Quincke subsequently asserted that there were great variations in the refractive relations of quartz, which appear to be somewhat dependent upon the age of the surface from which the total reflection was observed. He states that this difference reached two units of the second decimal place in some cases. Hallock has carefully examined a number of specimens, and the greatest difference that he found was less than one unit of the third place. This included the uncertainty of observation, which was equivalent to at least two or three units of the fourth place.—*Wiedem. Ann.* C.

Book Notices.

Working Drawings, and How to Make and Use Them, designed for Industrial, Technical, etc., Schools, and Artisans desiring a knowledge of the principles of Pattern and Template making. By Lewis M. Haupt, Professor of Civil Engineering in the University of Pennsylvania, etc. 12mo. Philadelphia: Jos. M. Stoddard & Co. 1880.

Engineers and master mechanics of all kinds have greatly felt the loss of time and effort expended in instructing their apprentices in the very rudiments of their professions and trades, and have been amazed that even graduates of technical and scientific schools generally come to them with entirely erroneous methods of thought and practice.

The title and preface of this book give the impression that the author has done a good work in presenting a plan by which cadet engineers can be graduated capable of entering the draughting room and being immediately useful, with their imaginations trained to clearly conceive an object from a sketch or verbal description; with the technical knowledge of scales, pencils, instruments, inks and colors, and the manual skill to use them with neatness and dispatch; and with

some experience of the best American practice in regard to the making of working drawings, such as the relative arrangement of different views, the avoiding of unnecessary repetitions, the judicious use of sections, coloring, and shade lines, and particularly with a knowledge of the best distribution of dimension lines and figures, and with a neat and clear style of making them. An examination of the text will fail to confirm this impression. Instead of teaching "How to Make and Use Working Drawings," and of being "the connecting link between theory and practice," it is based entirely on the theory of Descriptive Geometry and is in direct opposition to the practice of American and English engineers.

Davies' "Descriptive Geometry," published in 1826 [in which the author prefers no claim to invention or discovery, but merely to publication in America, and states that the subject has been taught since 1817 in the U. S. Military Academy, and that, in France, it was then considered indispensable to the architect and engineer] is more clear, interesting and useful than the book under review, which is a diffuse elaboration of the same old system, as applied to the projection of points, lines and planes.

The best engineers have long since discarded these theories and methods, with their diedral angles, ground lines, projections, traces, etc., as tending to confuse a subject which is essentially simple; and, with a full knowledge of these old problems, analyses, constructions and rules, have adopted the common sense method of showing objects as they appear when viewed from different sides, and of placing these views on the paper in their natural relative positions: thus, a top-view is placed above a front-view; a view of the right-hand side is placed to the right either of the front-view or of the top-view; a view of the left-hand side, to the left; and a view from underneath is placed below. The advantage of using this method in drawing a complicated machine is very great. It brings the different views of the same part closer together, facilitating the projection of the points, and the comprehension of the drawing when finished. It is the natural, logical method of procedure. A workman, seeing on a drawing a front view of a horizontal engine for instance, and wishing to get the dimensions of the crank, other than those shown in this view, would naturally expect to find them on the end-view shown next to the crank-end and not on the one next the cylinder-end, precisely as he would walk

around to that end of the engine itself to see the thickness or length of bearing of the crank.

The result of the teachings of text-books, such as this, is that the graduates of our schools, on entering practice, have to acquire a new method of comprehension, and a new manner of delineation, which, although far simpler and more natural than the old, is obtained at a disadvantage, in that the old one was received from what they have been taught to consider standard authorities.

A subject which is so simple, easy and interesting as the making of working drawings, and which could be made a relief to the tedium of studies, should not be put in the form of problem, theorem, analysis and construction, but should be based on the experience and practice of eminent engineers and artisans, and should form a training for the common sense and good judgment of students, and a preparation for their entrance to the designing room and workshop.

Even for teaching theoretical descriptive geometry the engineer's system would be simpler and clearer, and the advantage of adopting it in our technical and industrial schools would be very great, because the training of the students would then be in harmony with their future work, time and trouble would be saved during their early apprenticeship, and they and their employers would be spared the difficulty of overcoming erroneous early impressions and prejudices. A text-book of descriptive geometry, based upon the method of thought and mode of procedure actually adopted by skilled draughtsmen, and supplemented by one on working drawings as they are really made and used in our large industrial establishments, would deserve to be studied in every scientific school and by every mechanic in the country; but a professedly practical book, like the one under review, which teaches obsolete methods and incorrect technicalities, must certainly be misleading and injurious to the people whom it was intended to benefit.

W. H. T.

FRANKLIN INSTITUTE DRAWING SCHOOL.

The following is an abstract of Mr. Robert Grimshaw's remarks at the recent closing exercises of the school:

"I leave my own drawing-room for the purpose of doing myself the honor of attending the closing exercises of your drawing class; to

testify the interest I feel in instruction in drawing in general, and in the schools thus interested in particular.

"You have kindly asked me to speak in behalf of the art of drawing and of this particular school; but, although I note with interest that there are both free hand and mechanical drawings exposed here as the result of the year's course, permit me to narrow my remarks to the subject of mechanical drawing, concerning which I know the most and in which I am most interested.

"If printing be described as the art preservative of arts, drawing might with equal aptness be called the art definitive of arts. It is to the mechanical arts what notation is to music, algebra to mathematics. In its wonderful manifold qualities it is used for illustration, description, expression; for record and study; as an aid to invention, design, calculation, tabulation and generalization.

"As a means of illustration it stands unrivaled, conveying ideas in sequence, and logical demonstration where words would be lame or useless. Without it the college professor or the Franklin Institute lecturer would be lost. That it excels words in the matter of description is amply proved by its free use in descriptive circulars of machines and by its entry into columns of daily papers. As a means of record, our own Patent Office has recently recognized it as far superior to models, which have in most cases been dispensed with. It has this advantage over models that, while a model may be tampered with and show no signs of change, a drawing would, even if there were but one copy extant, be less easily altered; if altered it shows the fraud more plainly; and, besides this, there are likely to be duplicates or copies of the original drawing, which will attest any change or additions made in after years by any one to add to or detract from the value of the invention. As a means of study, who could learn anything about the slide valve, for instance, without diagrams? A few pencil lines relieve the imagination of the great strain necessary to picture the actual or relative positions of the moving and fixed parts. As an aid to invention, there are many who will certify how the friendly line has saved thought, time and expense, has preceded and helped along the birth of new movements and combinations, and revealed faults which would have been very expensive to remedy in the pattern, model, or actual machine. What is true of original invention is true of design for well-known devices.

"Of course, knowing the value of drawing, It is easy to see that it